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HYDROSTATICAL PARADOXES,

Made out by New
EXPERIMENTS,
(For the moſt part
PHYSICAL and Eaſie.)

By the Honourable
ROBERT BOYLE;
Fellow of the Royall Society.

OXFORD,
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THE PUBLISHERS ADVERTISEMENT TO THE READER.

WHen the Author Writ the following Treatiſe, he had a deſign, as appears by ſome paſſages in the Preface, to publiſh together with it ſome things, which he had divers years before provided for an Appendix to his

Phyſico-Mechanical Treatiſe about the Aire: But part of the Appendix conſiſting of Experiments, which the Authour has ſeveral times made, but truſting to his memory, did not think it neceſſary to Record, when he came to recollect particulars, he found that ſome years which had paſs'd ſince divers of them were try'd, and variety of intervening occurrents, had made it unſafe for him to rely abſolutely upon his Memory for all the circumſtances fit to be ſet down in the Hyſtorical part of the deſign'd Appendix. And therefore he reſolv'd to repeat divers Experiments and Obſervations, that he might ſet down their Phænomena whil'ſt they were freſh in his Memory, if not objects of his ſenſe. But though, when he Writ the following Preface, he did it upon a probable ſuppoſition, that he ſhould ſeaſonably be able to repeat the intended

Tryals, yet his Expectation was ſadly diſappointed by that heavy, as well as juſt, Viſitation of the Plague which happened at *London* whil'ſt the Author was in the Country: and which much earlier then was apprehended, began to make havock of the People, at ſo ſad a rate that not only the Glaſs-men there were ſcatter'd, and had, as they themſelves advertis'd him, put out their Fires, but alſo Carriers, and other ways of Commerce (ſave by the Poſt) were ſtrictly prohibited betwixt the parts he reſided in and *London*; which yet was the only place in *England* whence he could furniſh himſelf with peculiarly ſhap'd Glaſſes, and other Mechanical Implements requiſite to his purpoſes; And the ſame Calamity continuing ſtill, without yet affording us any certaine ground of determining when it will end: The Author chuſes rather to ſuffer the following Paradoxes to come abroad without the Appendix,(which is no way neceſſary to them, whatever they may be to It,) then any longer put off thoſe Ingenious perſons that ſolicited the publication of them.

THE

The PREFACE.

THe Riſe of the following Treatiſe being a Command impos'd on me by the Royal Society, the Reader will, I hope, need no more then this intimation, to keep him from wondering to find ſome paſſages worded as parts of a Diſcourſe pronounc'd before an Aſſembly, it being not unuſual (though not neceſſary) to preſent either in writing or by word of mouth, together with the Experiments made before that Illuſtrious Company, an Hiſtorical account of Them.

But becauſe 'tis probable, that ſome Readers will deſire to be ſatisfied about other particulars, relating to the publication of this Treatiſe, I preſume it will not be amiſs, both to ſay ſomething of the Rea-

ſons why I publiſh it as the firſt part of the preſent Appendix to my Phyſico-Mechanical Experiments, *and to give ſome account of the manner of writing it.*

I had quickly both an opportunity and an Invitation to enlarge the papers I was to read, beyond the Limits of a bare deſcription of the Phænomena, and matters of fact, by my having been through ſome intervening Accidents ſo hindered from exhibiting *them altogether, that I was deſir'd to bring in an Accompt in Writing, that might be Regiſtred (how little ſoever worthy of ſuch Company) in the Societies Collection of Philoſophical Papers, for the ſake of thoſe Members who could not be preſent at all the* Experiments *: So that finding ſome enlargements expected from me, I was eaſily induc'd to add the* Explications *of the Phænomena I deſcrib'd, whilſt I perceiv'd that by a ſmall addition of pains I might much gratifie divers Ingenious Friends that were not ſo well vers'd in* Hydroſtaticks *as in the other parts of real Learning.*

*Having thus been induc'd to enlarge the Account of my Experiments till it had attain'd the bulk 'tis now arriv'd at, I confeſs I was without much difficulty perſwaded, that to ſuffer it to paſs abroad * in the Company of the Appendix wherewith 'tis publiſh'd, would not prove unacceptable to the Curious, no more then an improper introduction to the reſt of my Appendix, and that for ſeveral Reaſons.*

** About this paſſage, See the Publiſher to the Reader.*

For (*firſt*) *the Hydroſtaticks is a part of Philoſophy, which I confeſs I look upon as one of the ingeniouſeſt Doctrines that belong to it. Theorems and Problems of this Art, being moſt of them pure and handſome productions of Reaſon duly exercis'd on attentively conſider'd Subjects, and making in them ſuch Diſcoveries as are not only pleaſing, but divers of them ſurpriſing, and ſuch as would make one at firſt wonder by what kind of Ratiocination men came to attain the knowledg of ſuch unobvious*

ous Truths. Nor are the delightfulneſs, and the ſubtilty of the Hydroſtaticks, *the only things for vvhich vve may commend Them: For there are many, as well of the more familiar, as of the more abſtruſe Phænomena of* Nature, *that will never be throughly underſtood, nor clearly explicated by thoſe that are ſtrangers to the* Hydroſtaticks; *upon whoſe Principles depend, beſides many other things, the Explications of moſt of the Phyſico-Mechanical Experiments, we have ventur'd to preſent the* Publick, *and the* Deciſion *of thoſe many* Controverſies, *which they, and the Phænomena of the Torrecellian Experiment have occaſion'd among the Modern Inquirers into* Nature.

But the uſe of this Art is not alone Speculative, but Practical, ſince not onely the propoſitions it teaches, may be of great importance to Navigation, *and to thoſe that inquire into the magnitudes and gravities of* Bodies, *as alſo to them that deal in Salt* *workes: But that the Hydroſtaticks may be made divers ways ſerviceable to the Chymiſts themſelves, to whoſe Art that Doctrine ſeems to be ſo little of* Kin, *I might here manifeſt, if I could think it fit to tranſcribe, what I have * elſewhere deliver'd to that purpoſe.*

* Chiefly, in ſeveral places of the unpubliſh'd part of the Treatiſe of the uſefulneſs of Experimental Philoſophy.

But that which invited me to Write ſomething of this part of Philoſophy, is, not only that I think it conſiderable, but that, notwithſtanding its being ſo, I find it but very litle, and not very happily cultivated. For being not look'd upon as a Diſcipline purely Mathematical, the generality of Mathematicians have not in their Writings ſo much as taken notice of it, much leſs improv'd It. And ſince the admirable Archimedes, *who, in his little Tract* De inſidentibus humido, *has left us three or four very excellent propoſitions, (but proved by no very eaſie Demonſtrations)*

ons) among divers others that have more of Geometrical Subtility, then uſefulneſs; Thoſe Mathematicians, that, (like Marinus Ghetaldus, Stevinus, *and* Galileo) *have added any thing conſiderable to the Hydroſtaticks have been (that I know of) very few, and thoſe too, have been wont to handle them, rather as Geometricians, then as Philoſophers, and without referring them to the explication of the Phænomena of Nature. And as for the Peripateticks, and other School-Philoſophers, though on ſome Occaſions, as when they tell us, That water weighs not in water, nor aire in aire, they deliver aſſertions about matters belonging to the Hydroſtaticks, (which term, in this Treatiſe, I often take in a large ſenſe becauſe moſt of the things delivered about the weight of Bodys may by eaſy variations, be made applicable to other Fluids) yet they are ſo far from having illuſtrated, or improv'd them, that they have but broach'd or* *credited, divers of the moſt erroneous conceits, that are entertain'd about them. So that, there being but few Treatiſes written about the Hydroſtaticks, and thoſe commonly bound up among other Mathematical works, and ſo written, as to require Mathematical Readers, this uſefull part of Philoſophy, has been ſcarce known any farther then by name, to the generality ev'n of thoſe Learned men, that have been inquiſitive into the other parts of it, and are deſervedly reckoned among the ingenious Cultivators of the modern Philoſophy. But this is not all, For ſome eminent men, that have of late years, treated of matters Hydroſtatical; having been prepoſſeſs'd with ſome errenous Opinions of the peripatetick School, and finding it difficult, to conſult experience, about the truth of their Concluſions, have interwoven divers erroneous Doctrines among the ſounder propoſitions, which they either borrow'd from* Archimedes, *and other circumſpect Mathematicians, or devis'd* *vis'd themſelves, and theſe miſtakes being deliver'd in a Mathematical dreſs, and mingled with Propoſitions demonſtrably true, the Reputation of ſuch Learned Men, (from which I am far from deſiring to detract,) and the unqualifiedneſs of moſt Readers, to examine Mathematical things, has procur'd ſo general an entertainment for thoſe Errors, that now the Hydroſtaticks is grown a part of Learning, which 'tis not only difficult to attain, but dangerous to Study.*

Wherefore, though neither the Occaſion and deſigne of this Treatiſe exacted, nor my want of skill and leaſure qualified me to Write either a Body or Elements of Hydroſtaticks: yet I hop'd I might doe ſomething, both towards the illuſtrating, and towards the reſcue of ſo valuable a Diſcipline, by Publiſhing the enſuing Tract; where I endeavour to diſprove the receiv'd errors, by eſtabliſhing Paradoxes contrary to them, and to make the Truths the

the better underſtood and receiv'd , partly *by a way of Explicating them unimploy'd in* Hydroſtatical Books, *and* partly *by confirming the things I deliver by Phyſical and ſenſible Experiments. And over and above this, the more to recommend* Hydroſtaticks *Themſelves to the Reader, I have, beſides the Paradoxes, oppos'd to the Errors I would diſprove, taken occaſion by the ſame way, to make out ſome of the uſefulleſt of thoſe* Hydroſtatical *Truths, that are wont to ſeem ſtrange to Beginners.*

If it be here demanded, why I have made ſome of my Explications ſo prolix, and have on ſeveral occaſions inculcated *ſome things. I anſwer, That thoſe who are not us'd to read Mathematical Books, are wont to be ſo indiſpos'd to apprehend things, that muſt be explicated by Schemes, and I have found the generality of Learned men, and ev'n of thoſe new Philoſophers that are not skill'd in Mathematicks, ſo much*

much more unacquainted, then I before imagin'd both with the principles and Theorems of Hydroſtaticks, *and with the ways of explicating and proving them, that I fear'd, that neither the Paradoxes themſelves, that I maintain, nor the* Hypotheſes *about the weight and preſſure of the aire, upon which, little leſs then my whole Pneumatical Book depends; would be throughly underſtood without ſuch a clear Explication of ſome* Hydroſtatical *Theorems, as to a perſon not vers'd in Mathematical writings, could ſcarce be ſatisfactorily deliver'd in few words. And therefore, though I do not doubt, that thoſe who are good at the moſt compendious ways of demonſtrating, will think, I might in divers places, have ſpar'd many words without injury to my proofs, and though I am my ſelf, of the ſame mind I exſpect to find them of; yet, I confeſs that 'twas out of choice that I declind that cloſe and conciſe way of writing, that in other caſes I am wont moſt* to eſteeem. For Writing now not to credit my ſelf, but to inſtruct others, I had rather Geometricians ſhould not commend the ſhortneſs of my Proofs, then that thoſe other Readers, whom I chiefly deſign'd to gratifie, ſhould not throughly apprehend the meaning of them.

But this is not all for which I am to excuſe my ſelfe to Mathematicall Readers. For ſome of them, I fear, will not like that I ſhould offer for Proofs ſuch Phyſical Experiments, as do not alwayes demonſtrate the things, they would evince, with a Mathematical certainty and accurateneſſe; and much leſs will they approve, that I ſhould annex ſuch Experiments to confirm the Explications, as if Suppoſitions and Schemes, well reaſon'd on, were not ſufficient to convince any rational man about matters Hydroſtaticall.

In Anſwer to this I muſt repreſent, that in Phyſical Enquiries it is often ſufficient that our determinations come very near the matter, though they fall ſhort of a Mathematical Exactneſs. And I chooſe rather to preſume upon the equity of the Reader, then to trouble him and my ſelf with tedious Circumlocutions, to avoid the poſſibility of being miſunderſtood, or of needing his Candor. And we ſee, that even Mathematicians are wont, without finding any inconvenience thereby, to ſuppoſe all perpendicular Lines, made by pendulous Bodies, to be parallel to one another: Though indeed they are not; ſince, being produc'd, they would meet at the Centre of the Earth: And to preſume, that the Surface of every calme water, in a Veſſel, is parallel to the Horizon; and conſequently, a Plain: Though, in ſtrictneſs, themſelves think it the portion of a Sphere: And though alſo I have u-

ſually obſerv'd it to be higher, where 'tis almoſt contiguous to the ſides of the Veſſel, then 'tis in other places.

Moreover, ſince we find that though water will be uniformly rais'd in Pumps to ſeveral heights, but not to thirty five foot, and will in ordinary open pipes, be almoſt of the ſame level within and without, but not if the pipe be extraordinary ſlender; Upon theſe, and divers other ſuch conſiderations, I may have ſometimes made uſe of expreſſions, that ſeemed not poſitive and determinate enough to be employed about matters to which Mathematical Demonſtrations are thought applicable. But I elſewhere give an account of the ſcruples I have about ſuch Demonſtrations, as they are wont to be apply'd to Phyſical matters. And, in the preſent Paradoxes, I think I have not done nothing, if in my Hydroſtatical *Explications I have made it appear, That in*

Experiments made with ſuch Liquors and Glaſſes, as I employed, the Rules will hold without any ſenſible, or at leaſt any conſiderable Error; for thereby we may learn the Truth of many things, for the main, though in ſome we ſhould not have attained to the exactneſs of meaſures and proportions, which yet our endeavors may aſſiſt others to arrive at.

And as for my confirmation of Hydroſtatical propoſitions by Phyſical Experiments, if ſome Readers diſlike that way, I make no doubt but that the moſt will not only approve it, but thank me for it. For though, in pure Mathematicks, he that can demonſtrate well, may be ſure of the Truth of a Concluſion, without conſulting Experience about it: Yet becauſe demonſtrations are wont to be built upon Suppoſitions or Poſtulates; and ſome things, though not in Arithmetick or Geometry, yet in Phyſical matters, are *wont to be taken for granted, about which men are lyable to ſlip into miſtakes; even when we doubt not of the Ratiocination, we may doubt of the concluſion, becauſe we may, of the Truth of ſome of the things it ſupppoſes. And this Conſideration, if there were no other, will, I hope, excuſe me to Mathematicians, for ventring to confute ſome reaſonings that are given out for Mathematical demonſtrations. For I ſuppoſe it will be conſider'd, that thoſe whoſe preſum'd Demonſtrations I examine, though they were ſome of them Profeſſours of Mathematicks, yet did not Write meerly as Mathematicians, but partly as Naturaliſts: ſo that to queſtion their Tenets, ought not to diſparage thoſe, as well certain, as excellent and moſt uſeful Sciences, pure Mathematicks, any more then that the Mathematicians that follow the Ptolemaick, the Copernican, the Tichonian, or other Syſtemes*

of

of the world, Write Books to manifeſt one anothers Paralogiſmes in Aſtronomical matters: And therefore (to proceed to what I was about to ſay) it cannot but be a ſatisfaction to a wary man to conſult ſenſe about thoſe things that fall under the Cogniſance of it, and to examine by Experiences, whether men have not been miſtaken in their Hypotheſes *and Reaſonings, and therefore the Learned* Stevinus *himſelf (the chief of the Moderne Writers of* Hydroſtaticks) *thought fit, after the end of his* Hydroſtatical *Elements, to add in an Appendix ſome Pragmatical Examples (as he calls them) that is, Mechanical Experiments (how cogent I now inquire not) to confirm the Truth of his Tenth Propoſition, to which he had, not far from the beginning of his* Book, *annexed what he thinks a Mathematical* Demonſtration. *And, about the very Subjects we are now upon, the following*

Paradoxes will diſcover ſo many miſtakes of eminent Writers, that pretend to have Mathematically demonſtrated what they teach, that it cannot but make wary Naturaliſts (and 'tis chiefly to gratifie ſuch that I publiſh this) be ſomewhat diffident of Concluſions, whoſe proofs they do not well underſtand. And it cannot but, to ſuch, be of great ſatisfaction to find the things, that are taught them, verified by the viſible teſtimony of Nature her ſelf. The importance of this Subject, and the frequent Occaſion I have to make uſe of this kind of Apology, will I hope, procure me the Readers pardon if I have inſiſted ſomewhat long upon it.

After what has been hitherto diſcours'd, 'twil be eaſie for me to give an Account, why I premiſed theſe Hydroſtatical *Paradoxes to the reſt of the Appendix, wherewith they are * now publiſh'd: For ſince a great part of my work in that Appendix, was to be a further Explication*

* An Account of this paſſage alſo, may be had from the Publiſhers Advertiſement to the Reader.

of ſome things delivered in the Book it is ſubjoyn'd to, and the vindication of them from invalid objections: And ſince I have generally obſerv'd, that the objections that have bin, either publickly or privately, made againſt the explications & reaſonings contain'd in that Book, were wont to proceed from unacquaintedneſs, either with the true notion of the weight and ſpring of the aire, as I maintain them, or with the Principles and Theorems of Hydroſtaticks, or elſe from erroneous Conceits about them; I thought it would much conduce to both the forementioned ends of my Appendix, If I clear'd up that Doctrine to which my Experiments and reaſonings have been all along Conſonant, & whoſe being either not known, or miſunderſtood, ſeems to have occaſion'd the objections that have been hitherto made againſt the Hypotheſes I have propos'd, or the Explications I have thence given. And however, ſince the Proofs I offer for my opinions are for the moſt part drawn from

Experiments new & eaſie, and that my aim is but to diſcover Truths, or make them out by clearer explications, without ſuppoſing, like thoſe I diſſent from, any thing that is either precarious *or* ſcarce, *if at all,* intelligible; *I hope, that if I ſhould not prove happy enough to reach my ends, yet the Ingenious and Equitable Reader will approve my Deſigne, and be advantaged by my Experiments. Of which ſome of the chiefeſt, and ſome of the moſt difficult, having been ſeen (divers of them more then once) by the* Royal Society *it ſelf, or by inquiſitive Members of it; it will, I preſume, be but a reaſonable requeſt, if the Reader, that ſhall have the curioſity to try them over again, be deſired not to be haſty in diſtruſting the matters of fact, in caſe he ſhould not be able at firſt to make every thing ſucceed according to expectation. For as eaſie as I have endeavour'd to make theſe Experiments, yet I dare not promiſe my ſelf that they will all of them be priviledg'd from the fate* *whereto I have obſerv'd other Phyſico-Mathematical ones to be not ſeldome obnoxious from ſome unheeded Phyſical Circumſtance, by which thoſe that are not acquainted with the ſubtleties of Nature, or, at leaſt for the time, do not ſufficiently conſider them, are apt to be impoſed upon.*

This Advertiſement will perhaps be beſt illuſtrated, & recommended by an inſtance. And therefore I ſhall ſubjoyne one that will poſſibly ſeem ſomewhat odd.

It has been taken notice of by two or three Ingenious modern Mathematicians, and I have had occaſion to make it out by particular Experiments, that warm water is lighter in ſpecie *then cold: whence it has been deduc'd, that wax, and other Bodies, very near æquiponderant with common water, will ſwim in that which is cold, and ſinck in that which is hot, or luke-warm. Which Experiment, though as it may be (and perhaps it has been) tryed, I readily allow to be agreeable to the known Laws of the Hydroſtaticks;*

Yet I have ſometimes undertaken that the Tryal ſhould have a quite contrary event. To this purpoſe having taken ſome yellow Bees-wax, *which was formed into a Pellet of the bigneſs of a Cherry, and, by the help of a little Lead, was made ſo near æquiponderant to cold water, that, being but a very little heavier, a very ſmall diminution of its weight would make it emerge, I remov'd it out of the very cold water, into ſome that had bin purpoſely made lukewarm, (or a little more then ſo) where it quickly, ſomewhat to the wonder of the lookers on, appeard to ſwim on the top of the water. And that it might not be ſuſpected that it was ſupported by any viſible bubbles, which I have obſerved, in ſome caſes, to buoy up even heavy Bodies, and deceive the unskilful, or unattentive; I briskly enough duck'd the bullet 2 or 3 times under water to throw them off, notwithſtanding which it conſtantly return'd to float, and yet being remov'd again into the ſame cold water it had been taken* *out of, and duck'd as before to free it from adherent bubbles, it lay quietly at the bottom, and, though rais'd ſeveral times to the upper part of the water, would immediately ſubſide again, and fall to the very loweſt. Now that w^{ch} invited me topromiſe an Experiment which ſeems to contradict the principles of the* Hydroſtaticks, *was not any diſtrust of thoſe principles themſelves, but a conjecture, that as by warmth the water would be made a little lighter* in ſpecie *then 'twas before; ſo by the ſame warmth the ſpirituous and more agitable parts of the wax, whoſe texture is looſe enough, would be ſomwhat (though not viſibly) expanded, and would by that expanſion gain a greater advantage towards floating, then the increas'd lightneſs of the water would give it diſpoſition to ſinck. And I confirm'd this conjecture by a farther experiment, which at firſt was it ſelf ſomewhat ſurpriſing to the Beholders. For when the wax was firſt taken out of the cold water, & immediately immers'd in the* *warm, it would readily enough ſinck, & being (with a quill or a knife) rais'd to the top of the water, it would again fall down, but more ſlowly then at the begining, & after ſome few minutes, if it were rais'd to the upper parts of the water, it would remain a float. (And I have known it, when it had remain'd a while longer at the bottom, ſo to emerge, that if I were ſure no unheeded bubbles had been newly generated, and held it up, it might be ſaid to emerge of its own accord) as on the other ſide, being put into the cold water as ſoon as ever it was taken out of the warm, it would at the very firſt float, and being then knock'd downwards, it would, readily enough, regain the upper part of the water, but if I continu'd to ſend it downwards about 6 or 7 times (more or fewer) ſucceſſively, it would emerge every time more ſlowly then other, and at length not emerge at all, even when I try'd it in water made heavy, by being highly infrigidated with ſalt aud ſnow plac'd about the Glaſs. Which Phænomena I had* *thought it reaſonable to expect, becauſe I preſum'd, that the Wax being remov'd immediately out of the warm water, into the cold, muſt require ſome time, to looſe the adventitious expanſion, which the warmth had given it, and muſt be depriv'd of it by degrees, by the coldneſs of the water into which the wax was transferr'd. As on the other ſide, there muſt be ſome time neceſſary for ſo little a warmth, as that of the tepid (or little more then tepid) water, to give the wax that addition of dimenſions (which alſo it muſt receive by degrees) that was neceſſary, in ſpite of the rarefaction of the water, to make it float. I might add, that theſe Tryals were repeated, for the main, with more Bullets of wax then one, and that they ſucceeded far otherwiſe, when, inſtead of a piece of wax, we imploy'd a pois'd glaſs bubble, in which the temperature could make either no change at all, or no conſiderable change of dimenſions. And to theſe I might add other circumſtances, if I did not remember,*

ber, that I mention theſe Tryals but occaſionally, and to make the caution, formerly recommended to the Reader, appear not to be impertinent, ſince a Hydroſtatical Experiment, true in its ſelf, may eaſily miſcarry by over-looking ſuch Circumſtances as 'tis not eaſie to be aware of.

But by this Advertiſement I would by no means divert Men from being diffident of Hydroſtatical Traditions and Experiments. For, beſides the many Erroneous Opinions, there are matters of fact, whoſe Truth, thô *not queſtion'd, but built upon, I think ought to be brought to tryal. For, even whilſt I was concluding this Preface, I found that divers even of the Moderns, & particularly a very learned Man that has lately Writen of Hydroſticks, have much troubled themſelves to render a reaſon why, ſince, according to their Doctrine, water weighs not in water, Wooden veſſels, though of a ſubſtance lighter then water, being by leaks, or otherwiſe, fil'd with water, ſhould ſinck and remain at* *the bottom of the water: whereas judging this* Phænomenon *diſagreable to what I look upon as the Laws of the Hydroſtaticks, I was confirm'd in that opinion, by having had the curioſity to make ſome tryals of it, with 4 or 5 veſſels of differing ſhapes and ſizes, whereof two were of wax, which, though a matter but very little lighter then water, I could not ſinck, or keep ſunck by pouring water into them, or ſuffering them to fill themſelves at leaks made near the bottom, and if they were depreſſed by force or weights, they, as alſo the wooden Veſſels, would upon the removal of the impediment (and ſometimes with the cavity upwards) emerge. And I am the more ſolicitous to have things in the Hydroſtaticks duly aſcertain'd, becauſe* the weighing of bodies in Liquors *may hereafter appear to be one of the general ways I have employ'd, and would recommend, for the examining of almoſt all ſorts of tangible Bodies.*

Paradox.1. That in Water and other Fluids, the lower parts are preſs'd by the upper. 24

Par.2. That a lighter Fluid may gravitate or weigh upon a heavier. 43

Par.3. That if a Body contiguous to the water be altogether, or in part, lower than the higheſt level of the ſaid water, the lower part of the Body will be preſs'd upward by the water that touches it beneath. 67

Par.4. That in the aſcenſion of water in Pumps, &c. there needs nothing to raiſe the Water, but a competent weight of an External Fluid. 94

Par.5. That the preſſure of an Externall Fluid is able to keep an Heterogeneous Liquor ſuſpended at the ſame height in ſeveral Pipes, though thoſe Pipes be of very different Diameters. 106

Par.6. If a Body be plac'd under water, with its uppermoſt Surface parallel to the Horizon; how much water ſoever there may be on this or that ſide above the Body, the direct preſſure ſuſtain'd by the Body (for we now conſider not the Lateral nor the recoyling preſſure, to which the Body may be expos'd if quite environ'd with water) is no more then that of a Columne of water, having the Horizontal ſuperficies of the Body for its Baſis, and the perpendicular depth of the water for it's height.

And ſo likewiſe,

If the water that leans upon the Body be contain'd in pipes open at both ends; the preſſure of the water is to be eſtimated by the weight of a pillar of water, whoſe Baſis is equal to the lower Orifice of the pipe, (which we ſuppoſe to be parallel to the Horizon) and its height equal to a perpendicular reaching thence to the top of the water; though the pipe be much inclin'd towards the Horizon, or though it be irregularly ſhap'd, and much broader in ſome parts, then the ſaid Orifice. 117

*Par.*7. That a Body immers'd in a Fluid, ſuſtains a lateral preſſure from the Fluid; and that increas'd, as the depth of the immers'd Body, beneath the Surface of the Fluid, increaſeth. 142

*Par.*8. That water may be made as well to depreſs a Body lighter then it ſelf, as to buoy it up. 160

*Par.*9. That, what ever is ſaid of poſitive Levity, a parcel of oyle lighter then water, may be kept in water without aſcending in it. 165

*Par.*10. That the cauſe of the Aſcenſion of water in Syphons, and of its flowing through them, may be explicated without having a recourſe to natures abhorrency of a Vacuum. 170

*Par.*11. That a ſolid Body, as ponderous as any yet known, though near the Top of the water, it will ſinck by its own weight; yet if it be plac'd at a greater depth then that of twenty times its own thickneſs, it will not ſinck, if its deſcent be not aſſiſted by the weight of the incumbent water. 184

Appendix. 1. Containing an Anſwer to ſeven Objections, propos'd by a late Learned Writer, to evince, that the upper parts of water preſs not upon the lower. 198

Ap. 2. Concerning the Reaſon why Divers, & others who deſcend to the bottom of the ſea, are not oppreſs'd by the weight of the incũbent water. 221

Imprimatur,

ROBERTUS SAY,
VICE-CANCELLARIUS
OXON.

HYDROSTATICAL PARADOXES,

Made out by

NEW EXPERIMENTS:

Preſented to the

ROYAL SOCIETY;

(*The* Lord Viſcount Brouncker *being then Preſident.*) *May* 1664.

My L O R D,

TO obey the orders of the Society, that forbid the making of Prefaces and Apologies in Accounts of the Nature of that which you expect from me; I ſhall without any further preamble begin with taking notice, that upon peruſal of *Monſieur Paſchall*'s ſmall French Book, which was put into my hands, I find it to conſiſt of two diſtinct Treatiſes: The one *of the Æquilibrium* of Liquors, as he calls it; and the other *of the weight of the Maſs of the Air*.

As for this latter, (which I ſhall mention firſt, becauſe I can in very few words diſpatch the little I have to ſay of it) Though it be an ingenious diſcourſe, and containes things, which if they had been publiſhed at the time, when it is ſaid to have been written, would probably have been very well-come to the Curious: yet I have very little elſe to ſay of it in this place, in regard that ſince that time, ſuch kind of Experiments have been ſo proſecuted, that I preſume it is needleſs, and would not be acceptable to repeat what *Monſieur Paſchall* has written, in this Society, which has ſeen the ſame

Truths, and divers others of the like Nature, more clearly made out by Experiments, which could not be made by *Monſieur Paſchall*, and thoſe other Learned Men, that wanted the advantage of ſuch Engines and Inſtruments, as have in this place been frequently made uſe of.

Wherefore having already at a former meeting given you, by word of Mouth, an account of *Monſieur Paſchall*'s Ingenious Invention, of a pair of Bellows without vent, to meaſure the various Preſſure of the Atmoſphære; I remember nothing elſe that needs hinder me from proceeding to the other part of his Book, *The Treatiſe of the Æquilibrium of Liquors.*

This I find ſo ſhort, and ſo worthy of the Author, that to give you all that I judge worth taking notice of in it, would obliege me to tranſcribe *almoſt* the whole Tract; and therefore I ſhall rather invite you to read the whole, then divert you from the deſigne by culling out any part of it; yet if you will not be ſatisfied without ſomething of more particular, I ſhall be oblig'd to tell you, That the Diſcourſe conſiſting partly of Concluſions and partly of Experiments; the former ſeemed to me to be almoſt all of them (there being but few that I doubt of) conſonant to the Principles and Lawes of the Hydroſtaticks. But as for the latter, the Experimental proofs he offers of his opinions are ſuch, that I confeſs I have no mind to make uſe of them.

And the Reaſons why, notwithſtanding that I like moſt of *Monſieur Paſchall*'s Aſſertions, I decline imploying his way of proving them, are principally theſe.

Firſt, Becauſe though the Experiments he mentions be delivered in ſuch a manner, as is uſual in mentioning matters of fact; yet I remember not that he expreſly ſays that he actually try'd them, and therefore he might poſſibly have ſet them down as things that *muſt* happen, upon a juſt confidence that he was not miſtaken in his Ratiocinations. And of the reaſonableneſs of this Doubt of mine, I ſhall ere long have occaſion to give an inſtance.

Secondly, Whether or no *Monſieur Paſchall* ever made theſe Experiments himſelf; he does not ſeem to have been very deſirous, that others ſhould make them after him. For he ſuppoſes the Phænomena he builds upon to be produc'd fifteen or twenty foot under water. And one of them requires, that a Man ſhould ſit there with the End of a Tube leaning upon his Thigh. But he neither teaches us how a Man ſhall be enabled to continue under water, nor how in a great Ciſtern full of water, twenty foot deep, the Experimenter ſhall be able to diſcern the alterations, that happen to *Mercury* and other Bodies at the Bottome.

And *Thirdly*, Theſe Experiments require not only Tubes twenty foot long, and a great Veſſel of at leaſt as many feet in depth, which will not in this Countrey be eaſily procured, but they require Braſs Cylinders, or Pluggs, made with an exactneſs, that, though eaſily ſuppoſed by a Mathematician, will ſcarce be found obtainable from a Tradeſman.

Theſe difficulties making the Experiments propos'd by *Monſieur Paſchall* more ingenious then practicable, I was induc'd on this occaſion to bethink my ſelf of a far more Expeditious Way, to make out, not only moſt of the Concluſions wherein we agree, but others that he mentions not; and this with ſo much more eaſe and clearneſſe, That not only This Illuſtrious Aſſembly, but perſons no more than moderately vers'd in the Vulgar principles of the Hydroſtaticks, may eaſily enough apprehend what is deſign'd to be deliver'd; if they will but bring with them a due Attention, and minds diſpos'd to preferre Reaſon and Experience to vulgar Opinions and Authors; which laſt clauſe I annex, becauſe the following Diſcourſe, pretending to confute ſeveral of thoſe, challenges a right to except againſt their Authority.

It not being my preſent Task to deliver the Elements, or a Body of Hydroſtaticks, but only ten or twelve Paradoxes, which I conceive to be proveable by this new way of making them out, I ſhall, to avoid Confuſion, Deliver Them in as many diſtinct propoſitions; After each of which, I ſhall indeavour in a proof, or an Explication, to ſhow, both that it is true, and why it ought to be ſo. To all theſe I ſhall to avoid needleſs Repetitions, premiſe a word or two by way either of *poſtulatum* or *Lemma*.

And becauſe I remember to what Aſſembly I addreſs This Diſcourſe, I ſhall make uſe of no other then an eaſie ſuppoſition I met with in a ſhort Paper (about a Mercuriall Phænomenon) brought in a year or two ſince to this Learned Society, by a deſervedly Famous Member of it*, For though his ſuppoſal be made upon occaſion of an Experiment of another Nature, then any of the enſuing, it may be eaſily accomodated to my preſent purpoſe.

* *That excellent Mathematician the Learned Dr* Wallis, Savilian Profeſſor of Geometry.

This *poſtulatum* or *Lemma*, conſiſts of three parts; the firſt of them more, and the two laſt, leſs principal.

Suppoſe we then, (*Firſt*) That if a *Pipe* open at both Ends, and held perpendicular to the Horizon, have the lower of them under Water, there paſſes an Imaginary plain or Surface, which touching that Orifice is parallel to the Horizon; and conſequently parallel as to ſenſe to the upper Surface of the water, and this being but a help to the Imagination will readily be granted.

Secondly, To this it will be conſonant, that each part of this deſignable ſurface, will be as much, and no more preſs'd, as any other equal part of it, by the water that is perpendicularly incumbent on it. For the water or other Fluid being ſuppoſed to be of an homogeneous ſubſtance, as to gravity, and being of an equal height upon all the parts of the imaginary Surface; there is no reaſon why one part ſhould be more preſs'd by a perpendicular pillar of that incumbent fluid, then any other equal part of the ſame Surface by another perpendicularly incumbent pillar of the ſame or equal Baſis and height, as well as of the ſame Liquor.

But *Thirdly*, Though whilſt our imaginary Surface is equally preſs'd upon in all parts of it, the Liquor muſt retain its former poſition; yet if any one part comes to have a greater weight incumbent on it, then there is upon the reſt, that part muſt be diſplac'd, or depreſs'd, as it happens, when a ſtone or other Body heavier then water ſincks in water. For wherever ſuch a a Body happens to be underneath the water, that part of the imaginary plain that is contiguous to the lower part of the ſtone, having on it a greater weight then other parts of the ſame Surface, muſt needs give way, and this will be done ſucceſſively till the ſtone arrive at the Bottom; and if, on the other ſide, any part of the Imaginary Surface be leſs preſs'd upon then all the reſt ; it will by the greater preſſure on the other parts of the Surface be impell'd upwards, till it have attain'd a height, at which the preſſure (of the rais'd water, and the lighter or floating Body (if any there be) that leans upon it, and gravitates together with it, upon the ſubjacent part of the Imaginary Surface) will be equal to that which bears upon the other parts of the ſame Surface.

And becauſe this ſeems to be the likelieſt thing to be Queſtion'd in our Aſſumption, though he that conſiders it attentively, will eaſily enough be induc'd to grant it: Yet I ſhall here endeavour to evince it Experimentally, and that by no other way of proof, then the ſame I imploy all along this preſent diſcourſe.

This Experiment and the Explication of it, if to ſome they ſhould here ſeem ſomewhat obſcure, will be eaſily underſtood by the Figures and Explications belonging to the firſt enſuing Paradoxe.

Take then a Cylindrical glaſs pipe, of a convenient Bore open at both Ends, let the Tube be ſteadily held perpendicular to the Horizon, the lower end of it being two or three inches beneath the Surface of a convenient quantity of water, which ought not to fill the Glaſs Veſſel that contains it. The pipe being held in this poſture, 'tis manifeſt, that the water within the pipe, will be *almoſt* in a level with the Surface of the water without the pipe, becauſe the external and internal water (as I am wont for Brevities ſake to call them) have free intercourſe with one another by the open Orifice of the immers'd End of the pipe: yet I thought fit to inſert the word *almoſt*, becauſe if the pipe be any thing ſlender, the Surface of the water in it, will always be ſomewhat higher then that of the water without it, for reaſons that 'tis not ſo neceſſary we ſhould now inquire after ; as 'tis , that we ſhould here deſire to have this taken notice of once for all ; That miſtakes may be avoided without a troubleſome repetition of the difference in heights of the Surface of Liquors within pipes and without them , in caſe they be any thing ſlender.

The pipe being held in the newly mention'd poſture , if you gently poure a convenient Quantity of Oyle upon the external water , you ſhall ſee, That as the Oyle grows higher and higher above the Surface of That water , the water within it , will riſe higher and higher , and continue to do ſo , as long as you continue to poure on oyle ; Of which the Reaſon ſeems manifeſtly to be this ; That in the Imaginary plaine that paſſes by the Orifice of the immers'd end of the pipe , all that is not within the Compaſs of the Orifice , is expos'd to an additional preſſure from the weight of the oyle which ſwims upon the water , and that preſſure muſt ſtill be increas'd , as there is more and more oyle poured on ; whereas a Circular part of the Imaginary plain, equal to the Orifice of the Glaſſe , is by the ſides of the pipe fenc'd from the immediate preſſure of the oyle ; ſo that all thoſe other parts of the water , being far more preſs'd , then that part which is comprehended within the Cavity of the Tube : and conſequently the preſs'd parts of the external water, are by the equal gravitation of the oyle , upon the parts of the external water , impell'd up into the Cavity of the pipe, where they find leſs reſiſtance , then any where elſe , till they arrive at ſuch a height , that the Cylinder of water, within the pipe , do's as much gravitate upon the ſubjacent part of the Imaginary Surface , as the water and oyle together , do upon every other equal part of the ſame Surface or plain.

But as well the former *Lemma*, as this Experiment, will be ſufficiently both clear'd and confirm'd by the following Explications; to which I ſhould for that Reaſon forthwith proceed; Were it not that, ſince divers paſſages of the following Treatiſe ſuppoſe the Aire to be a Body not devoid of weight, which yet divers Learned adherents to the Peripatetick Philoſophy do reſolutely deny, it ſeems requiſite to premiſe ſomething for the proof of this Truth.

And though I think the Arguments we have imploy'd to that purpoſe already, do ſtrongly evince it: yet if I may be allow'd to anticipate one of my own Experiments of the Appendix, I ſhall give an inſtance of the weight of the Aire, not lyable ſo much as to thoſe invalid objections, which *ſome of* the Ariſtotelians have made againſt thoſe

Proofs, wherewith we have been ſo happy, as to ſatisfie the learned'ſt even of our profeſſed Adverſaries.

We caus'd then to be blown at the flame of a Lamp, a Bubble of glaſs, (of about the bigneſs of a ſmall Hen-egge) which, that it might be light enough to be weigh'd in exact Scales, ought to be of no greater thickneſs, then is judged neceſſary to keep it from being (when ſeal'd up with none but very much expanded aire in it) broken by the preſſure of the ambient Atmoſphære. This bubble was (like a Peare with its ſtemme) furniſh'd with a very ſlender pipe of Glaſs, at which it was blown, that it might be readily ſeald up; and then (the Aire within it being by the flame of the Lamp gradually rarified, as much as conveniently could be) whilſt the Body of the Bubble was exceeding hot, the newly mentioned ſtemme was nimbly put into the middle of the flame; where, by reaſon of its ſlenderneſs, the Glaſs, which was exceeding thin, was immediately melted; whereby the Bubble was Hermetically ſeal'd up. This Glaſs being permitted leaſurely to coole, I could afterwards keep it by me an hour, or a day, or a week, or longer, if I thought fit; and when I had a mind to ſhew the Experiment, *I* put it in one of the ſcales of an exact ballance, that would turn, perhaps with the 30*th*, or 50*th*, or a leſſe part of a grain; and having carefully counterpois'd it, *I* then warily broke off the ſeal'd end, placing a ſheet of paper juſt under the ſcale to receive the fragments of the Glaſs: and putting in again thoſe fragments, that ſcale wherein the Glaſs was would conſiderably preponderate; which it muſt do upon the account of the Weight of Aire, there being no other cauſe, either needful, or juſtly aſſignable, but the weight of the Aire that ruſh'd into the Cavity of the Glaſs, as finding leſs reſiſtance there then elſewhere, by reaſon that the included Aire had it's ſpring much weakn'd by it's great expanſion.

This Experiment I many times tryed, ſometimes before ſome *Virtuoſi*, and ſometimes before others; who all allowed it to be concluſive. For here it could not be objected as againſt the weighing of Aire in a Bladder, (which objections yet I could eaſily anſwer, if it were *now* proper) that the aire which ponderates, is ſtuff'd with the Effluvia of him that blows the Bladder, and (beſides that) is not aire in its Natural ſtate, but violently compreſs'd. For here 'tis the free aire, and in it's wonted laxity, that makes the Glaſs preponderate.

And that there is a great Ingreſs of

the external aire, is evident by theſe *three* Phænomena. The *one*, that if you lend an attentive Ear, you ſhall plainly heare a kind of whiſtling noiſe to be made by the external aire, as it ruſhes violently in upon the breaking of the Glaſs; The *other*, that the Rarefaction of the aire, ſeal'd up in the bubble, being very great, there is a great deal of ſpace left for the ambient aire to fill upon its admiſſion; and the greatneſs of this Rarefaction may be gueſs'd at, both by the breaking of ſuch bubbles now and then by the preſſure of the External aire, which is not competently aſſiſted by the Internal to reſiſt; and alſo by the *third* Phænomenon I intended to take notice of, namely, That if, inſtead of breaking off the ſeal'd end of the Glaſs in the aire, you break it under water, that Liquor will, by the Preſſure of the Atmoſphære, be forc'd to ſpring up like an artificial Fountaine into the Cavity of the Bubble, and fill about three quarters of it. By which laſt circumſtance I gather, that the weight of the aire is more conſiderable then ev'n many, who admit the aire to have weight, ſeem to imagine. For we muſt not ſuppoſe, that all the aire contain'd in the Bubble, when broken, weighs no more then the weight requiſite in the oppoſite Scale, to reduce the Ballance to an Æquilibrium; ſince this additional weight is onely that of the aire, that intrudes on the breaking of the glaſs; which aire, by the Obſervations newly mention'd to have been made with water, appears to be but about three quarters of the whole aire contain'd in the broken Bubble; and yet, according both to our Eſtimate, and that of divers Virtuoſi, and ſome of them eminent Mathematicians, when the capacity of the Bubble was ſhort of two cubical Inches, (and ſo proportionably

tionably in other glaſſes,) the nice Ballance we us'd, manifeſted the newly admitted Aire to amount to ſome times near halfe a grain, and ſometimes beyond it.

And becauſe one of the laſt Experiments that I made to this purpoſe, with ſeal'd Bubbles was none of the leaſt accurate, I ſhall conclude this Subject with the following account of it.

A thin glaſs Bubble, blown at the flame of a Lamp, and Hermetically ſeal'd when the contained aire was exceedingly rarified, was Counterpoiz'd in a nice paire of Scales, and then the ſeal'd *apex* being broken off, and put again into the ſame Scale, the weight appear'd to be increas'd by the re-admitted aire, a pretty deal above $\frac{11}{16}^{\text{ths}}$, and conſequently very near, if not full $\frac{3}{4}$ of a graine: Laſtly, having by ſome ſlight (for 'tis no very eaſie matter) fill'd it with common water, we weigh'd the glaſs and water together, and found the latter, beſides the former, to amount to 906 grains: ſo that ſuppoſing, according to our former Eſtimate, countenanced by ſome Tryals, that the readmitted aire, which amounted to $\frac{3}{4}$ of a grain, fill'd but $\frac{3}{4}$ of the whole Cavity of the Bubble, the aire that was in it, when ſeal'd, poſſeſſing one quarter of that Cavity, the whole aire contain'd in the Bubble, may be reaſonably preſum'd to weigh a whole grain; in which caſe we might conclude (abſtracting from ſome little Niceties not fit to be taken notice of here, as elſewhere) that the water in our Experiment, weighed very little more then nine hundred times as much as an equal quantity of Aire, And therefore, though we allow, that in an Experiment ſo diligently made, as this was, the aire præexiſtent in the bubble did not adæquately poſſeſs ſo much as a fourth part, but about a fifth or a ſixth of its Cavity, the aire will yet appear ſo heavy, that this Experiment will agree well with thoſe others, recorded in another Treatiſe, wherein we aſſign'd (*numero rotundo*) a thouſand to one, for the proportion wherein the ſpecifick Gravity of water exceeds that of aire.

PARA-

PARADOX I.

That in Water, and other Fluids, the lower parts are preſs'd by the upper.

PRovide a Glaſs veſſel of a convenient height and breadth A. B. C. D. fill'd with water almoſt to the Top; Then take a glaſs Pipe, open at both Ends, Cylindrical, and of a ſmall Bore, (as about the eighth or ſixth part of an Inch in Diameter.) Put the lower End of this Pipe into clear Oyle or Spirit of Turpentine; and having by Suction rais'd the Liquor to what part of the Pipe you think fit, as ſoon as it is there, you muſt, very nimbly removing your Lips, ſtop the upper Orifice with the pulp of your finger, that the rais'd Liquor may not fall back again: Then taking the Pipe and that Liquor out of the Oyle of Turpentine, place it perpendicularly in the Glaſs of water, ſo as that the Surface of the Oyle in the Pipe be ſomewhat higher then that of the water without the Pipe; and having ſo done, though you take off your finger from the upper Orifice of the Pipe, the Oyle will not fall down at the lower Orifice, though that be open, but will remain ſuſpended at the ſame height, or near there abouts, that it reſted at before.

Now Oyle of Turpentine, being a heavy Fluid, does, as ſuch, tend downwards, and not being ſtopp'd by the Glaſs it ſelf, whoſe lower Orifice is left open, it would certainly fall down through the Pipe, if it were not kept ſuſpended by the preſſure (upwards) of the water beneath it. There appearing no other Cauſe to which the Effect can reaſonably be aſcrib'd, and this being ſufficient to give an Account of it, as we ſhall preſently ſee. For that it is not any contrariety in Nature, betwixt the oyle and the water, as Liquors that will not mingle, is evident from hence, That if you had remov'd your finger when the Pipe was not ſo deeply immers'd in the Glaſs, but that the Surface of the oyl in the Pipe was an Inch or two more elevated above that of the water in the Glaſs, then in our preſent caſe we ſuppoſe it to be; The Oyle, notwithſtanding its preſum'd contrariety to water, would have freely ſubſided in the Pipe, till it had attain'd an æquipollency of preſſure with the External Water.

The Reaſon therefore of the Phænomenon ſeems to be plainly this. Suppoſing the imaginary ſurface, on which the Extremity *Q* of the pipe *P Q* leans, to be *G H*. If that part of the Surface, on which the Oyl leans at *Q*, be as much, and no more charged, or preſs'd upon by the weight of the incumbent Cylinder of Oyle Q X, then the other parts of the ſame imaginary Surface G H are by the water incumbent on Them, there is no Reaſon why that part at Q ſhould be diſplac'd, either by being depreſs'd by the weight of the Cylinder of Oyle X Q, or rais'd by the equal preſſure of water upon the other parts of the Superficies G H.

And that this *Æquilibrium*, betwixt the Oyle and the Water, is the true cauſe of the Phænomenon, may be confirm'd by obſerving what happens, if the altitude of either of the two Liquors be alter'd in Relation to the other.

And (*Firſt*,) we have already taken notice, That if the Cylinder of Oyle reach in the Pipe, much higher then that of the Surface of the water, the oyle will deſcend: Of which the Reaſon is, Becauſe the deſignable Surface G H, being more charg'd at Q then any where elſe, the part *Q*, being unable to reſiſt ſo great a preſſure, muſt neceſſarily be thruſt out of place by the deſcending oyle.

Secondly, This ſubſiding will continue but till the Surface of the Oyle in the Pipe be fallen almoſt as low as that of the water without the Pipe; becauſe then, and not before, the parts at *Q* are but as much preſs'd by the oyle, as the other parts of the Surface *G H* are by the water that leans upon them.

Thirdly, 'Tis a concluding Circumſtance to our preſent purpoſe, That if the Oyle and Water being in an *Æquilibrium*, you gently lift up the Pipe, as from *Q* to *S*, the depth of the water being leſſend, the oyle in the Pipe will grow præponderant, and therefore will fall out in Drops or Globuls, which by the greater Specifick Gravity of the water, will be buoy'd up to the Top of the Liquor, and there flote: And ſtill as you liſt up the Pipe higher and higher, towards the Surface *L M*, more and more of the Oyle will run out. But if you ſtop the Pipe any where in its Aſcent, as at *S*, the Effluxion of the oyle will likewiſe be ſtopp'd. And at the imaginary Superficies *J* K, as by Reaſon of the ſhallowneſs of the water from *L* to *J*, or *M* to K, the preſſure of the water upon the other parts of the Surface is not near ſo great, as it was upon the Surface *G H*, where the water had a greater depth : So by reaſon of the proportionate Effluxion of the oyle, whil'ſt the Pipe was lifted up from *Q* to *S*, the remaining Cylinder of oyle incumbent on *S*, is not able to preſs that part of the Superficies *J* K more ſtrongly then the other parts of the ſame Superficies, are preſt by the water Incumbent on them. And if the Pipe be lifted up till the lower Orifice be almoſt rais'd to *V*; that is, almoſt as high as the uppermoſt Surface of the water *L M*, ſo much of the oyle will, for the Reaſon already given, run out, that there will ſcarce be any left in the Pipe *T V*.

Fourthly, But if when the Pipe reſts at the Surface *G H*, where the oyle is in an *Æquilibrium* with the water, you ſhould inſtead of lifting it from *Q* to *S*, thruſt it down from *Q* to *O*; then the External water would not only ſuſtaine the oyle, but make it aſcend in the Pipe to a height equal to the diſtance *E G*; and ſo the Pipe will containe beſides a longer Cylinder of oyle *Æ W*, a ſhorter one of water *Æ O*. For the pipe being transferr'd from the poſition *P Q*, to the poſition *O N*, there is a new Imaginary Surface *E F*, that paſſes by the lower Orifice of the Pipe. Now the part of this Surface at *O* will not, by the Incumbent oyle alone, be preſs'd as much as the other parts of the ſame

Surface are by the Incumbent water. For the oyl alone was but in *Æquilibrium* with the water, when it was no deeper then *L G*, or *H M*; ſo that the other parts of the Superficies *E F*, being more preſs'd upon by the water, then the part at *O* by the oyle, the oyle muſt give place, and be buoy'd up by the water, (which, if it were not for the weight of the oyle, would be impell'd up into the pipe full as high as the Surface of the External water) till the preſſure of the admitted water O *Æ*, and the Cylinder of oyle *Æ W*, do both together gravitate as much upon the part *O*, as the reſt of the Incumbent water does upon the other parts of the ſame Superficies *E F*.

Fifthly and laſtly, 'Tis very agreeable to what has been delivered, touching the *Æquilibrium* of the oyle and water in the pipe *P Q*, that the Surface *X* of the oyle in the pipe, will not be of the ſame level with *L M*, that of the External water, but a little higher than it. For though the ſlenderneſs of the Pipe do ſomewhat contribute to this Effect, yet there would be an inequality, though not ſo great, betwixt theſe Surfaces upon this Account, That oyle of Turpentine being in *Specie*, (as they ſpeak in the Schools) that is bulk for bulk, a lighter Liquor than Water, it is requiſite that the height of it, incumbent on the part *Q*, be greater than that of the water on the other parts of the ſame Surface *G H*, to make the preſſure of the oyle on the part it leans upon, equal to the preſſure of the water on the other parts of the Surface. And if the inequality were greater betwixt the Specifick Gravities of theſe two Liquors, the inequalities betwixt the Surface *X*, and the Surface *L M* would be alſo greater, as may be try'd by ſubſtituting for common water, oyle of Tartar *per deliquium*, which is a ſaline Liquor much heavier than it. And that, in caſe the Pipe containe not a lighter Liquor then the External fluid, the Surface of the Liquor in the Pipe will not be higher than that of the Liquor without it, we ſhall by and by have opportunity to manifeſt by Experience.

From what has been hitherto ſhewen, we may ſafely infer the Propoſition, upon whoſe occaſion all this has been delivered. For ſince the oyle in a Pipe, open at both Ends, may be kept ſuſpended in any part under water, as at *Q*, becauſe it is there in an *Æquilibrium* with the External water; and ſince being lifted up in the water, as from Q to *S*, the oyle can no longer be kept ſuſpended, but by its own gravity will runne out. And ſince, in a word, the deeper the water is, the greater weight and preſſure is requir'd in the Cylinder of oyle,

oyle,

oyle, to be able to countervail the preſſure of the water, and keep it ſelf from being lifted up thereby; there ſeems no cauſe to doubt but that the parts of the water incumbent on the Superficies *G H*, do more preſs that Superficies, than the parts of the water contiguous to the Superficies *J K* do preſs that; and conſequently, that the parts of the water that are under the uppermoſt Surface of it, are preſs'd by thoſe of the ſame Fluid that are directly over them: As we ſaw alſo that the upper parts of the oyle, whil'ſt the pipe was in raiſing from *Q* to *S*, depreſs'd the lower ſo much, as to force them quite out of the Pipe; there being in theſe caſes no reaſon why the lowermoſt parts of a Liquor ſhould preſs more, or have a ſtronger Endeavour againſt any other Liquor (or any other Body) the higher the Liquor incumbent reaches, if theſe inferiour parts deriv'd their peſſure only ly from their own particular Gravity, (which is no greater then that of the other Homogeneous parts of the Liquor) and therefore they muſt derive the great force wherewith they preſs from the weight of the Incumbent parts, which conſequently muſt be allow'd to preſs upon them.

But before I proceed to the following propoſitions, it will not be amiſs to mention here, once for all, a few advertiſements, to avoid the neceſſity of repeating the ſame things in the ſequel of the Diſcourſe.

And *Firſt*, What is here ſaid of the preſſure of the parts of water upon one another, and the other Affections that we ſhall attribute to it, in the following paper, are to be apply'd to heavy Fluids in general, unleſs there ſhall appear ſome particular Cauſe of excepting ſome of them in particular Caſes.

Secondly, Whereas I lately intimated, That the inequality betwixt the Surfaces of the oyle in the Pipe, and of the External water, was in part to be aſcrib'd to the ſlenderneſs of the Pipe, to be imploy'd in theſe Experiments, I did it for this cauſe, that, whatever the Reaſon of it be, (which we need not here inquire after,) we are aſſur'd by Experience, as we have elſewhere ſhewn, That when Glaſs pipes come to be ſlender, water and many other Liquors (though not Quickſilver) will have within them a higher Surface then that of the ſame Liquor without them, and this inequality of Surfaces (as far as we have yet try'd) increaſes with the ſlenderneſs of the pipe. But this, as to our preſent Experiment, is a matter of ſo little moment, That it may ſuffice to have intimated that we did not overſee it.

Thirdly, Wherefore, notwithſtanding this little inconvenience of ſlender

Glaſſes, we think it Expedient to imploy ſuch in the following Experiments, becauſe we found, that in thoſe of a wide Bore, upon ſuch little inequalities of preſſure as are not eaſily to be avoided, the oyle and water will paſs by one another in the Cavity of the pipe, and ſo ſpoile the Experiment, which requires that the oyle within the pipe be kept in an intire and diſtinct Body.

Fourthly, Common oyle and water, or any other two Liquors that will not mingle, may ſerve the turn in moſt of theſe Experiments; but we rather chuſe oyle of Turpentine, becauſe it is light and thin, clear and colourleſs, and may be eaſily had in quantities, and is not ſo apt to ſpot ones Cloaths, or obſtinately to adhere to the porous Bodies it chances to fall on, as Common, and other expreſs'd oyles. And for their ſakes to whom the odour is offenſive, we preſently correct it, by mingling with it a convenient quantity of oyle of Rhodium, or ſome other Chymical oyle that is odoriferous.

Fifthly, Oyle of Turpentine, though it be not reckon'd among the ſaline Menſtruums, will yet (as we elſewhere note) work upon Copper, and ſo by digeſting it upon crude filings of that Metal, we obtaine a deep green Liquor, which may be made uſe of inſtead of the Limpid oyle, to make the Diſtinction of the Liquors more conſpicuous.

Sixthly, And for the ſame purpoſe we often uſe inſtead of clear water, a ſtrong Decoction of Brazill, or Loggwood, or elſe Red Inck it ſelf. I ſay, a *ſtrong Decoction*, becauſe unleſs the Liquor be ſo deeply ting'd, as to appear Opacous in the Glaſs, when it comes into the ſlender pipe, its Colour will be ſo diluted, as to be ſcarce diſcernable.

Seventhly, In the ſhape of the Glaſs Veſſel, we need not be Curious; though that of a wide Mouth'd Jarr, expreſs'd in the Scheme, be for ſome uſes more convenient than other ſhapes. The depth of theſe Glaſſes, and the length of the Pipes muſt be determin'd by the Experiments, about which one means to imploy them. To make out the firſt Paradox already prov'd, a Glaſs of about five or ſix Inches deep, and a Pipe about as many Inches long, will ſerve the turn: but for ſome others of the following Experiments, tall Cylindrical Glaſſes will be requiſite; and for ſome, Broad ones likewiſe will be Expedient.

Eighthly, One muſt not be diſcourag'd by not being able at the firſt or ſecond time, to ſuck up oyle of Turpentine to the due height, and ſtop it with ones finger from relapſing; but one muſt try again, and again; eſpecially ſince many Tryals of this kind may be made in a few Minutes: and for Beginners 'tis a ſafe and good, though not the ſhorteſt way, to ſuck up rather more Liquor then one judges will be needful; becauſe having fill'd the Pipe to that height, you may by letting in the Aire warily and ſlowly, between the Orifice of the Glaſs and the pulp of your finger, ſuffer ſo much Liquor to run out of the Pipe, as will reduce it to the height you deſire; and there, by cloſe ſtopping the Orifice with your finger, you may keep it ſuſpended as long as you pleaſe, and immerſe it into any Heterogeneous Liquor, and take it out again at pleaſure without ſpilling any of it. By which ſlight Expedient alone, I can decline ſeveral Difficulties, and do many things, which, according to *Monſieur Paſchal*'s way, require a great deal of Trouble and Apparatus to be perform'd.

Laſtly, In ſuch Experiments where it may be of uſe, That there be a conſiderable diſparity betwixt the two unmingled Liquors, we may (as is above intimated) inſtead of fair water, imploy *Oleum Tartari per deliquium*, and tinge it with Brazill or Chochineele; from either of which, but eſpecially from the latter, it will obtaine an exceeding deep Redneſs: and where one would avoid ſtrong ſents and oylineſs, he may, if he will be at the Charge, imploy oyle of Tartar *per deliquium*, inſtead of fair water, and highly Rectified Spirit of Wine, inſtead of oyle of Turpentine. For theſe two Liquors, though they will both readily mingle with water, will not with one another; and if a great quantity of ſome other Liquor be to be ſubſtituted for ſimple water, when theſe Chymical Liquors are not to be had in plenty, one may imploy (as we have done) a very ſtrong Solution made of Sea-ſalt, and filtred through Cap-paper: this Brine being near about as Limpid as common water, and farre heavier than it. And for a Curioſity, we have added to the two lately mentioned Liquors (oyle of Tartar, and Spirit of Wine) ſome oyl of Turpentine, and thereby had three Liquors of different Gravities, which will not by ſhaking, be brought ſo to *mingle*, as not quickly to part again, & retire each within its own Surface; and by thruſting a Pipe with water in the bottom of it (placing alſo ones finger upon the upper Orifice) beneath the Surface of the lowermoſt of theſe Liquors, and by opportunely raiſing or depreſſing it, one may ſomewhat vary the Experiment in a way not unpleaſant, but explicable upon the ſame grounds with the reſt of the Phænomena mentioned in this Diſcourſe.

PARADOX. II.

That a lighter Fluid may gavitate or weigh upon a heavier.

I Know that this is contrary to the common opinion, not only of the Schools, but ev'n of divers hodiern Mathematicians, and Writers of Hydroſtaticks; ſome of whom have abſolutely rejected this Paradox, though they do but doubt of the truth of the former.

But when I conſider, that whether the cauſe of Gravity be the pulſion of any ſuperior ſubſtance, or the Magnetical attraction of the Earth, or whatever elſe it be, there is in all heavy Bodies, as ſuch, a conſtant tendency towards the Centre, or lowermoſt parts of the Earth; I do not ſee why that tendency or endeavour ſhould be deſtroy'd by the interpoſition of any other heavy Body; Though what would otherwiſe be the effect of that endeavour, namely an approach towards the Centre, may be hindred by another Body, which being heavier then it, obtains by its greater gravity a lower place; but then the lighter Body tending downwards, muſt needs preſs upon the heavier that ſtands in its way, and muſt together with that heavier preſs upon whatever Body it is that ſupports them both, with a weight conſiſting of the united gravities of the more, and the leſs heavy Body.

But that which keeps Learned Men from acknowledging this Truth, ſeems to be this, That a lighter Liquor (or other Body) being environ'd with a heavyer, will not fall down but emerge to the Top; whence they conclude, that, in ſuch Caſes, it is not to be conſidered as a heavy, but as a Light Body.

But to this I anſwer, That though in Reſpect of the heavier Liquor, the leſs heavy may in ſome ſence be ſaid to be light; yet, notwithſtanding that relative or Comparative Levity, it retains all its abſolute Gravity, tending downwards as ſtrongly as before; though by a contrary and more potent Endeavour upwards of the contiguous liquor (whoſe lower parts, if leſs reſiſted, are preſſed upwards by the higher elſewhere incumbent; according to the Doctrine partly delivered already, and partly to be cleared by the proof of the next propoſition,) its endeavor downward is ſo ſurmounted that it is forcibly carry'd up. Thus when a piece of ſome light wood being held under water, is let go and ſuffer'd to emerge, though it he buoy'd up by the water, whoſe ſpecifick Gravity is greater, yet ev'n whilſt it aſcends it remains a heavy Body;

Body ; ſo that the aggregate of the water & the aſcending wood weighs more then the water alone would doe ; And when it floats upon the upper part of the water, as part of it is extant above the ſurface , ſo part of it is immerſt beneath it , which confirms what we were ſaying, That a lighter Body may gravitate upon a heavier.

And thus there is little doubt to be made but that if a man ſtand in one of the ſcales of a Ballance with a heavy ſtone ty'd to his hand , and hanging freely by his ſide , if then he lift that weight as high above his head as he can , notwithſtanding that the ſtones motion upwards makes it ſeem a light Body in reſpect of the Man whoſe Body it leaves beneath it , yet it dos not, either during its' aſcent or after, looſe any thing of its connatural weight. For the Man that lifts it up ſhall feel its tendency downwards to continue, though his force,being greater than that tendency, be able, notwithſtanding that tendency, to carry it up: and when it is aloft, it will ſo preſs againſt his hand, as to offend, if not alſo to bruiſe it; and the Stone, and the Man that ſupports it, will weigh no leſs in the Scale he ſtands in, then if he did not at all ſupport it, and they were both of them weigh'd apart.

Likewiſe, if you put into one Scale a wide mouth'd Glaſs full of water, and a good quantity of pouder'd common Salt; and into the other Scale, a Counterpoiſe to them both; you may obſerve, that, though at the beginning the Salt will manifeſtly lie at the bottome, and afterwards by degrees be ſo taken up into the Body of the Liquor, that not a grain will appear there; yet neverthe leſs (as far as I can judge by my Experiments) the weight in that Scale will not be diminiſhed by the weight of as much Salt as is inceſſantly either carried up, or ſupported by the reſtleſs motion of the diſſolving Corpuſcles of the water; but both the one and the other, (allowing for what may evaporate) will concurrently gravitate upon the Scale that the glaſs containing them leans on.

But of this more elſewhere. Now to prove the propoſition by the New Method, we have propos'd to our ſelf in this Diſcourſe.

Take a ſlender Glaſs pipe, and having ſuck'd up into it fair water, to the height of 3 or 4 Inches, ſtop nimbly the upper Orifice with your finger, and immerſe the lower into a Glaſs full of oyle of Turpentine, till the Surface of the oyle in the Veſſel be ſomewhat higher than that of the water in the Pipe; then removing your finger, though the Pipe do thereby become open at both Ends, the water will not fall down, being hinder'd by the preſſure of the oyle of Turpentine. As will be obvious to them that have attentively conſider'd the Explication of the former Paradox; there being but this difference between this Experiment and that there Explain'd, that here the water is in the Pipe, and the oyle in the Veſſel, whereas there the oyle was in the Pipe, and the water in the Veſſel. And if you either poure more oyle into the Glaſs, or thruſt the Pipe deeper into the oyle, you ſhall ſee that the water will be buoyed up towards the top of the Pipe; that is, a heavier Liquor will be lifted up by a lighter. And ſince, by the Explication of the firſt Propoſition, it appears, that the Reaſon why the Liquor is in this caſe rais'd *in* the Pipe, is the Gravity of the Liquor that raiſes it, we muſt allow that a lighter Liquor in *ſpecie*, may by its gravity preſs againſt a heavier.

And it agrees very well with our Explication, both of this, and of the firſt Experiment; that as there, the Surface of the oyle in the pipe was always higher than that of the water without it, becauſe the oyle being the lighter Liquor, a greater height of it was requir'd to make an *Æquilibrium*; ſo in our preſent Experiment, the Surface of the Liquor in the Pipe will alwayes be lower than that of the oyle without it. For in the imaginary plain * *E F*, the Cylinder of water *J G*, contain'd in the Pipe *J H*, will, by reaſon of its greater gravity, preſs as much upon the part *J*, as the diſtill'd oyle (K *E*, *J L*,) being a lighter Liquor, can do upon the other parts of the ſame ſuppos'd plain *E F*, though the oyle reach'd to a greater height above it.

See the ſecond Figure.

This ſecond Paradox, we have hitherto been diſcourſing of, may be alſo ſo prov'd by what we formerly deliver'd, to make out the Truth of the third part of the Lemma premiſed to theſe Propoſitions.

But becauſe this and the former Paradox are of importance, not only in themſelves but to the reſt of this Treatiſe, and are likely (in moſt Readers) to meet with indiſpoſition enough to be receiv'd, I will ſubjoyn in this place a couple of ſuch Experiments, as will not, I hope, be unacceptable; that I devis'd, the one to confirm this ſecond Paradox, and the other to prove the firſt.

Some of the Gentlemen now preſent may poſſibly remember, that about the end of the Year that preceded the two laſt, I brought into this place a certain new Inſtrument of Glaſs, whereby I made it appear, that the upper parts of water gravitate upon the lower; which I did by ſinking a Body, that was already under water, by pouring more water upon it.

But that Experiment belonging to other papers, I ſhall here ſubſtitute another perform'd by an Inſtrument, which though it makes not ſo fine a ſhew, may be more eaſily provided, and will as well as that other (though you were pleas'd to command that from me) ſerve to make out the ſame Truth; which I ſhall apply my ſelf to do, as ſoon as I have, by an Improvement of the Expedient I am to propoſe, made good my late promiſe of confirming the ſecond Paradox.

And before I can well draw an Argument from theſe Experiments, for either of the propoſitions to be prov'd by them, I muſt briefly repeat what I have elſewhere deliver'd already (on another occaſion) touching the cauſe of the ſincking

In certain Notes upon ſome of the Phyſico-mechanical Experiments, touching the Aire.

ing of ſuch Bubbles. Namely that the Bubble X conſiſting of Glaſs, which is heavier in *ſpecie* then Water; and Aire, which is lighter in *ſpecie* then Water; and, if you pleaſe, alſo of Water itſelf, which is of the ſame ſpecifick Gravity with Water; as long as this whole aggregate of ſeveral Bodys is lighter then an equal bulk of Water, it will float; but in caſe it grows heavier then ſo much water, it muſt, according to the known Laws of the Hydroſtaticks, neceſſarily ſinck, (being not otherwiſe ſupported.) Now when there is any competent preſſure (whether produc'd by weight or otherwiſe,) upon the water, in which this Bubble is for the moſt part immers'd, because the glaſs is a firm Body, & the water, though a Liquor, either ſuffers no compreſſion, or but an inconſiderable one; the Aire included in the Bubble, being a ſpringy

Fig. 3.

and very compreſſible Body , will be compell'd to ſhrink , and thereby poſſeſſing leſs Room, then it did before, the contiguous water will ſucceed in its place ; which being a body above a thouſand times heavier then aire , the Bubble will thereby become heavier then an equall Bulk of water , and conſequently will ſink : but if that force or preſſure be remov'd , the Impriſon'd Aire will by its own Spring free it ſelf from the intruding water ; and the Aggregate of Bodys,that makes up the Bubble, being thereby grown lighter then an equal bulk of water,the ſubſided bubble will preſently emerge to the Top.

This Explication of the Cauſes of the ſinking of Bubbles agrees, in ſome things, with the Doctrine of the Learned Jeſuites *Kercher* & *Shottus*, and ſome other writers, in the Acount they give of thoſe two Experiments that are commonly known by the name, the one of the Romane, the other of the Florentine Experiments. But there are alſo particulars wherein I (who have never a recourſe to a *fuga Vacui,*) diſſent from their Doctrine ; the principles I go upon, having invited and aſſiſted me to make that Experiment, afford me ſome new Phænomena, which agree not with their Opinions, but do with mine : but I forbear to mention them here, becauſe they belong to other Papers ; and for the ſame reaſon I omit ſome acceſſion of Ludicrous Phænomena (as they call them,) which I remember I have ſometimes added to thoſe, which our Induſtrious Authors have already deduc'd from thoſe Experiments.

Theſe things being premis'd, I proceed to the confirmation of the ſecond Paradox, by the following Experiment.

Take a long glaſs pipe, ſeal'd or otherwiſe exactly ſtop'd at one end and open at the other ; (whoſe Orifice if it be no wider, then that it may be conveniently ſtop'd with a mans Thumb, the Tube will be the fitter to exhibit ſome other Phænomena.) Into this pipe pour ſuch a quantity of common water, as that there may be a foot, or half a yard, or ſome other competent part left unfill'd, for the uſe to be by and by mention'd. Then having poiz'd a glaſs Bubble with a ſlender neck, in ſuch a manner as that though it will keep at the Top of the water, yet a very little addition of weight will ſuffice to ſinck it, put this Bubble thus poiz'd into the Tube ; where it will ſwim in the upper part of the water, as long as it is let alone, but if you gently pour oyle of Turpentine upon it, (I ſay gently to avoid confounding the Liquors) you will perceive that, for a while, the

Bubble will continue where it was: but if you continue pouring on oyl, till it have attain'd a ſufficient height above the water, (which 'twill be eaſie to peceive, becauſe thoſe two liquors will keep themſelves diſtinct) you ſhall ſee the Bubble ſubſide till it fall to the Bottom, and continue there as long as the oyl remains at the height above the water.

The Reaſon of this Phænomenon, according to our Doctrine, is this, That the oyl of Turpentine, though a lighter Liquor then water, yet gravitates upon the ſubjacent water, and by its preſſure forces ſome of it into the cavity of the bubble at the open Orifice of its neck, whereby the Buble, which was before but very little leſs heavy then an equal Bulk of water, being by this acceſſion made a little more heavy, muſt neceſſarily ſinck; and the cauſe of its ſubmerſion, namely the preſſure of the oyle, continuing, it muſt remain at the bottom.

And to confirm this explication I ſhall add, that in caſe, by inclining the Tube or otherwiſe, you remove the Cylinder of oyl, or a competent part of it, (in caſe it were longer then was neceſſary,) the Bubble will again emerge to the Top of the water (for, as for the oyle, that is too light a Liquor to buoy it up;) which happens only becauſe the preſſure of the oyle upon the water being taken of, the Air, by vertue of its own ſpring, is able to recover its former Expanſion, and reduce the bubble to be as light as 'twas before.

And now we may proceed to that other Experiment, by which we lately promis'd to confirm the firſt Paradox. And in ſome regard this following Experiment has been preferr'd, as more ſtrange, to that I have been reciting.

ting. For it ſeem'd much leſs improbable, that of two Heterogeneous Liquors, the inferior ſhould be preſs'd upon by the incumbent, which, though lighter, kept in an intire body above it, then that in water, which is a Homogeneous Liquor, and whoſe parts mingle moſt freely and exquiſitely with one another, the upper part ſhould preſs upon the lower; and that they will do ſo, may appear by the Experiment it is now time to ſubjoyn.

Provide a long Tube and a poiz'd Bubble, as in the former Experiment, then having pour'd water into the Tube, till it reach above 5 or 6 Inches (for a determinate height is no way neceſſary) above the Bottome, caſt in the Bubble, which will not only ſwim, but if you thruſt it down into the water it will of it ſelf emerge to the upper part of it. Wherefore take a ſlender Wand, or a Wire, or a ſlender glaſs pipe, or any ſuch Body that is long enough for your purpoſe, and with it having thruſt the bubble beneath the Surface of the water, pour water ſlowly into the Tube (whoſe Cavity will not be near fill'd by the rod or wire) till it have attain'd a competent height, (which, in my laſt Tryals, was about a Foot, or half a Yard above the bubble:) and you ſhall ſee, that the bubble, which before endeavour'd to emerge, will by the additional weight of the incumbent water, be depreſs'd to the bottom of the Tube. After which you may ſafely remove the wire, or other body that kept it from riſing. For as the weight of the Incumbent water was that which made it ſinck, ſo that weight continuing on it, the bubble will continue at the bottom.

But yet it is not without cauſe, that we imploy a wire, or ſome ſuch thing, in this Experiment, though we affirm it to be onely the weight of the Incumbent water, that makes the Bubble ſinck. For if you ſhould pour water into the Tube, to the height lately mention'd, or ev'n to a greater, if you did not make uſe of the Wire, it would not ſerve the turn; becauſe that as faſt as you pour in the water, the Bubble being left to it ſelf, will riſe together with it; and ſo, keeping always near the upper part of the water, it will never ſuffer the Liquor to be ſo high above it, as it muſt be, before it can depreſs it. But to confirm, that 'tis the weight of the Superior water that ſincks the Bubble, and keeps it at the Bottom; if you take out of the Tube a competent quantity of that Liquor, and ſo take of the preſſure of it from the Bubble, this will preſently, without any other help, begin to ſwim, and regain the upper part of the water; whence it may at pleaſure be præcipitated, by pouring back into the Tube the water that was taken out of it. And theſe Confirmations, added to the former Proofs of the firſt and ſecond Paradoxes, being we conceive ſufficient to ſatisfie Impartial Readers of the Truth of them, we ſhould preſently advance to the next Propoſition, if we did not think fit to interpoſe here a *Scholium.*

SCHOLIUM.

IT may perchance be wondred at, why, ſince we lately mention'd our having made ſome Tryals with oyle of Tartar *per deliquium*, we did not in the preſent Experiment, in ſtead of fair water, make uſe of that, it being a very much heavier Liquor, and (though it may be incorporated with expreſs'd oyles) unmingleable in ſuch Tryals with oyle of Turpentine. But to this I anſwer,

ſwer, That ev'n in ſuch ſlender pipes, as thoſe made uſe of about the firſt Experiment, I found that oyle of Tartar was ponderous enough to flow down, though ſlowly, into the oyle of Turpentine at one ſide of the immers'd Orifice, whilſt the oyle paſs'd upwards by it along the other ſide of the pipe. And my knowledge of this could not but make me a little wonder, That ſo Curious a perſon, as *Monſieur Paſchall*, ſhould ſomewhere teach, That if a Tube of above 14 foot long, and having its Orifice placed 14 foot under water, be full of Quickſilver, the fluid Metal will not all run out at the Bottom of the pipe, though the Top of it be left open to the Aire, but will be ſtop'd at a foot high in the pipe. For the Impetus, that its fall will give it, muſt probably make it flow quite out of the pipe: And, not here to mention thoſe Tryals of ours with Quickſilver and ſlender

Tubes, that made me think this very improbable, if we conſider that the Experiment will not ſucceed with much more favourable circumſtances, betwixt oyle of Turpentine and oyle of Tartar, though the heavier of theſe two Liquors be many times lighter than quickſilver: It tempts me much to ſuſpect, that *Monſieur Paſchall* never actually made the Experiment, at leaſt with a Tube as big as his Scheam would make one gueſs, but yet thought he might ſafely ſet it down, it being very conſequent to thoſe Principles, of whoſe Truth he was fully perſwaded. And indeed, were it not for the impetus, the Quickſilver would acquire in falling from ſuch a height, the Ratiocination were no way unworthy of him.

But Experiments that are but ſpeculatively true, ſhould be propos'd as ſuch, and may oftentimes fail in practiſe; becauſe there may intervene divers other things capable of making them miſcarry, which are overlook'd by the Speculator, that is wont to compute only the conſequences of that particular thing which he principally conſiders; As in this caſe our Author ſeems not to have conſider'd, that in ſuch Tubes, as the Torricellian Experiment is wont to be made in, the largneſs of them would make them unfit for this Tryal.

And I have known Ingenious men, that are very well exercis'd in making ſuch Experiments, complaine, that they could never make this of *Monſieur Paſchall*'s to ſucceed. In which attempts, that the ſize of the Tubes much contributed to the unſuccesfulneſs of the Tryals, I ſhall (without repeating what has been already intimated to that purpoſe) in the following part of this Diſcourſe have opportunity to manifeſt ; and withal to adde as Illuſtrious a proof of this our ſecond Paradox, as almoſt any we have yet given.

PARADOX III.

That if a Body contiguous to the water be altogether, or in part, lower than the higheſt level of the ſaid water, the lower part of the Body will be preſs'd upward by the water that touches it beneath.

THis may be prov'd by what has been already delivered in the Explication of the firſt Experiment: For where ever we conceive the loweſt part of the Body, which is either totally, or in part, immers'd in water, to be, there the imaginary Superficies being beneath the true Superficies, every part of that imaginary Superficies muſt be preſs'd upwards, by vertue of the weight of the water incumbent on all the other parts of the ſame Superficies, and ſo that part of it, on which the immers'd Body chances to leane, muſt for the ſame Reaſon have an endeavour upwards. And if that Endeavour be ſtronger then that wherewith the weight of the Body tends downwards, then (ſuppoſing there be no Accidental Impediment) the Body will be buoy'd or lifted up. And though the Body be heavier then ſo much water, and conſequently will ſubſide, yet that Endeavour upwards of the water, that touches its lower part, is onely rendred ineffectual to the raiſing or ſupporting the body, but not deſtroyed; the force of the heavy Body being from time to time reſiſted, and retarded by the water, as much as it would be if that Body were put into one Scale, and the weight of as much water, as is equal to it in bulk, were put into the other.

To confirm this, we may have recourſe to what we ſaid in the Explication of the ſecond Experiment. *Fig.* 1, 2. For in caſe the ſlender pipe, wherein the water is kept ſuſpended, be thruſt deeper into the oyl, or in caſe there be more oyle pour'd into the Veſſel, the water will be impell'd up higher into the pipe; which it would not be, if the oyle, though bulk for bulk a lighter Body, did not preſs againſt the lower Surface of the vvater, (where, alone, the two Liquors are contiguous,) more forcibly then the water by its gravity tends dovvnvvards. And even vvhen the Liquors reſt in an *Æquilibrium*, the oyle continually preſſes upvvards, againſt the lower Surface of the water; ſince in that continual endeavour upvvards conſiſts its conſtant reſiſtance to the continual endeavour that the gravity of the water gives it to deſcend. And ſince the ſame Phænomenon happens; whether we ſuſpend water in oyle, as in the ſecond Experiment, or oyle in water, as in the firſt: it appears, that the propoſition is as well applicable to thoſe caſes, where the ſuſtein'd Body is ſpecifically heavier, as to thoſe where 'tis ſpecifically lighter then the ſubjacent fluid.

But a further and clearer proof of this Doctrine will appear in the Explication of the next propoſition. In the mean time, to confirm that part of our Diſcourſe, where we mention'd the Reſiſtance made by the water to Bodies that ſinck in it, Let us ſuppoſe, in the annexed Figure, That the pipe *E F* contains an oyle *Fig.* 4. ſpecifically heavier then water, (as are the oyls of Guaiacum, of Cinnamon, or Cloves, and ſome others,) and then, That the oyle in the pipe, and the water without, being at reſt in an *Æquilibrium*, the pipe be ſlowly rais'd towards the Top of the Veſſel. 'Tis evident, from our former Doctrine, and from Experience too, that there will run out drops of oyle, which will fall from the bottom of the pipe, to that of the Veſſel; but far more ſlowly then if they fell out of the ſame pipe in the Aire.

Now to compute how much the preſſure of the water againſt the lower parts of the drop amounts too, let us ſuppoſe the drop to be *G*, to whoſe lowermoſt part there is contiguous, in any aſſignable place where it falls, the imaginary Superficies *H J*. 'Tis evident, That if the drop of oyle were not there, its place would be ſupplied by an equal bulk of water; which being of the ſame ſpecifick Gravity with the reſt of the water in the Veſſel, the Surface *H J* would be laden every where alike; and conſequently no part of it would be diſplac'd. But now,

the

the drop of oyle being heavier then ſo much water, that part of the imaginary ſuperficies, on which that drop leans, has more weight upon it, then any other equal part of the ſame Superficies; and conſequently, will give place to the deſcending drop. And ſince the caſe of every other ſuppos'd Surface, at which the drop can be conceiv'd to arrive in its deſcent, will be the ſame with that of the Superficies *H J*; it will for the Reaſon newly given, continue falling till it comes to the bottom of the Veſſel which will ſuffer it to fall no further. And in caſe the drop *G* were not, as we ſuppoſe it, of a ſubſtance heavier in *ſpecie* then water, but juſt equal to it, the contiguous part of the Superficies *H J* would be neither more nor leſs charged then the other parts of the ſame Supeficies; and the part lean'd on would be neither depreſs'd nor rais'd, but the drop *G* would continue in the ſame place. And ſo we may prove, (what is affirm'd by *Archimedes* , and other Hydroſtatical Writers) That a Body æquiponderant *in ſpecie* to water, will reſt in any aſſignable place of the water where 'tis put.

And (to proceed further) ſince, if the drop *G* were of a matter but æquiponderant to water it would not ſinck lower at all , no more then emerge ; it follows, that though being heavier in *ſpecie* then water, it will fall , yet the gravity upon whoſe account it falls, is no more then that by which it ſurmounts an equal bulk of water ; (ſince, if it were not for that overplus, the reſiſtance of the water would hinder it from falling at all :) and conſequently, it looſes in the water juſt as much of the weight it would have in the aire, as ſo much water, weigh'd likewiſe in the ſame aire, would amount to.

Which is a Phyſicall Account of

that grand Theorem of the Hydroſtaticks, which I do not remember that I have ſeen made out in any Printed Book, both ſolidly and clearly; The Learned *Stevinus* himſelf, to whom the later Writers are wont to refer, having but an obſcure (and not Phyſical) demonſtration of it.

And, becauſe this Theorem is not only very noble, but (as we elſewhere manifeſt) very uſeful, 'twill not be amiſs to add, That it may eaſily be confirm'd by Experiment.

For if you take (for inſtance) a piece of Lead, and hang it by a Horſe haire (that being ſuppos'd very near æquiponderant to water) from one of the Scales of an exact Ballance; and, when you have put a juſt Counterpoize in the other Scale, ſuffer the Lead to ſinck in a veſſel of water, till it be perfectly covered with it, but hangs freely in it, the counterpoize will very much preponderate. And, part of the Counterpoize being taken out till the Ballance be again reduc'd to an *Æquilibrium*, you may eaſily (by ſubducting what you have taken out, and comparing it with the whole weight of the Lead in the aire) find what part of its weight it looſes in the water. And then if you weigh any other piece of the ſame Lead, ſuppoſe a Lump of 12 *ounces*, and hang it by a Horſe haire at one ſcale, you may be ſure that by puting into the other ſcale a weight leſs by a twelfth part, (ſuppoſing Lead to water to be as twelve to one) that is eleven *ounces*, though the weights be farr from an *Æquilibrium* in the Aire, they will be reduc'd to it when the Lead is cover'd with water.

The preſſure of water againſt the lower part of the Body immers'd in it may be confirmed by adding; That we may thence deduce the cauſe of the emergency of wood and other Bodyes lighter then water; which though a familiar Effect, I have not found its cauſe to have been ſo much as enquired into by many, nor perhaps to have been well rendred by any. If we ſuppoſe then that the pipe be almoſt fill'd, not with a ſincking but a ſwimming oyle, as oyle of Turpentine, if, as in the firſt Experiment, the lower orifice be thruſt under water, (to a far leſs depth then that of the oyle in the pipe) and the upper be ſlovvly unſtop'd, the oyl vvill (as vve formerly declar'd) get out in drops at the bottom of the pipe. But to determine vvhy theſe drops, being quite cover'd and ſurrounded vvith vvater, and preſs'd by it as vvell dovvnvvards as upvvards, ſhould rather emerge then deſcend, I ſhall not content my ſelf to ſay, that vvater *in ſpecie* heavier then this kind of oyle; For, beſides that in ſome caſes (e're long to be mention'd) I have made the water to depreſs ev'n this kind of oyle, and beſides that 'tis not every piece of wood lighter in *ſpecie* then water that will float upon water, how ſhallow ſoever it be: The Queſtion is how this præpollent Gravity of the water comes to raiſe up the oyle, though there be perchance much more water, for it to break its way thorough, above it, then beneath it.

The Reaſon then of the emerſion of Lighter Bodies in heavier fluids, ſeems to be this, That the endeavour upwards of the water, contiguous to the lower part of the Body, is ſtronger then the endeavour downwards of the ſame Body, and the water incumbent on it. As, in the former Scheme, ſuppoſing the Drop *G* to be the oyle of Turpentine, and to touch the two imaginary and parallel plains *H J*, *K L*; 'tis evident, that upon the lower part of the Drop, *N*, there is a greater preſſure of water, then upon the upper part of the ſame Drop, *M*: becauſe that upon all the ſurface K *L*, there is but an uniform preſſure of water *A* K *B L*, and upon all the parts of the ſurface *H I*, there is a greater weight of water *A* H *B I*, except at the part *N*; for there the oyle G, being not ſo heavy as ſo much water, the oyle being expos'd to a greater preſſure from beneath, then its own gravity (and that of the water incumbent on it) will enable it to reſiſt, muſt neceſſarily give way and be impell'd upwards. And the caſe being the ſame between that and any other parallel plain, whereſoever we ſuppoſe it to be in its aſcent, it muſt conſequently be impell'd further and further upwards till it arrive at the Top; and there it will float upon the water. Or, (to Explicate the matter without Figures) when a ſpecifically lighter Body is immers'd under water, it is preſs'd againſt by two pillars of water; the one bearing againſt the upper, and the other againſt the lower part; and becauſe the lengths of both theſe Pillars muſt be computed from the Top of the water, the lower part of the immers'd body muſt be preſs'd upon by a Pillar longer then the upper part by the thickneſs of the immers'd Body; and conſequently muſt be preſs'd more upwards then downwards. And by how much the greater diſparity of ſpecifick Gravity there is betwixt the water and the emerging Body, by ſo much the ſwifter (*cæteris paribus*) it will aſcend: becauſe ſo much the more will there be of preſſure upon all the other parts of the imaginary ſurface, then upon that part that happens to be contiguous to the *Bottom* of the aſcending Body.

And upon the ſame Grounds we may give (what we have not yet met with) a good ſolution of that Probleme, propos'd by Hydroſtatical Writers, why, if a Cylindrical ſtick be cut in two parts, the one as long again as the other, and both of them, having been detain'd under water at the ſame depth, be let go at the ſame time and permitted to emerge, the greater will riſe faſter then the leſſer. For ſuppoſe one of theſe Bodies, as *O P*, to be two foot high, and the other, *Q R*, to be half ſo much, and that the lowermoſt Surfaces of both be in the ſame imaginary plaine, parallel to the uppermoſt ſurface of the water and three foot diſtant from it; in this caſe there will be againſt the lower part of each of the wooden Bodies a preſſure, (from the laterally ſuperior water) equal to that upon all the other parts of the Imaginary plain, whereto thoſe Bodies are contiguous; But whereas upon the upper ſurface of the ſhorter Body, *Q R*, there will lean a pillar of water two foot high, the pillar of the ſame Liquor that will lean upon the Top of the taller Body, *P O*, will be but one foot high ; as the attentive conſiderer will eaſily perceive. So that the wooden Bodys being lighter *in ſpecie* then water, both of them will be impell'd upwards; but that compounded pillar, (if I may ſo call it) which conſiſts of one foot of wood and two foot of water, will by its gravity more reſiſt the being rais'd, then that which conſiſts of two foot of wood and but one foot of water : ſo that the cauſe of the unequal celerity in the Aſcenſion of theſe Bodys conſiſts chieflly, (for I would neither overvalue nor exclude Concomitant Cauſes) that the difference of the preſſure againſt the upper and lower part of each body reſpectively is greater in one then in the other.

And hence we may probably deduce a reaſon of what we often obſerve in the

Diſtillation of the oyles of Anniſeeds, Cloves, and diverſe Aromatick vegetables, in Lembecks by the intervention of water ; for oftentimes, when the fire has not been well regulated , there will come over, beſides the floating Oyle, a whitiſh water , which will not in a long time become cleare. And as we have elſwhere taught, That whiteneſs to proceed from the numerous reflections from the oyly ſubſtance of the Concrete, by the heat of the fire broken into innumerable little Globuls, and diſpers'd through the Body of the water ; ſo the reaſon why this whiteneſs continues ſo long , ſeems to be *chiefly*(for I mention not ſuch things,as, the great ſurfaces that theſe little Globuls have in reſpect of their Bulk)that, becauſe of the exceeding minuteneſs of theſe Drops, the height of the water that preſſes upon the upper part, is almoſt equal to that of the water that preſſes againſt the lower part ; ſo that the difference between theſe two preſſures being inconſiderable, it has power to raiſe the Drops but very ſlowly,(inſomuch that upon this ground I deviſ'd a Menſtruum, wherewith I could mingle oyle in drops ſo exceedingly minute, that, ev'n when there was but a few ſpoonfuls of the mixture, it would continue whitiſh for divers whole days together) though at length they will emerge ; and the ſooner, becauſe whilſt they ſwim up and down , as they frequently chance to meet and run into one another , they compoſe greater Drops ; which are (for the Reaſon already given) leſs ſlowly impell'd up by the water : at the Top of which, the Chymiſt, after a due time, is wont to find new oyl floating. But whether this be any way applicable to the ſwimming of the inſenſible particles of corroded metals in *Aquafortis*, and other ſaline

Menſtruums, I muſt not now ſtay to enquire.

One thing more there is, that I would point at before I diſmiſs this Paradox; Namely, that, for the ſame Reaſon we have all this while deduc'd, when the emergent drop, or any other Body, floats upon the Top of the water, it will ſinck juſt ſo far, (& no farther) till the immers'd part of the floating Body be equal in Bulke to as much water as is equal in weight to the whole Body. For ſuppoſe, in the annexed figure, *Y* to be a Cube of wood three foot high and ſix pound in weight; this wood, being much heavier then Aire, will ſinck into the water, till it come to an imaginary ſuperficies, *X W*, where, having the poſition newly deſcrib'd, it will neceſſarily acquieſce. For all the other equal parts of the Superficies, *X*, *W*, *Q*, being lean'd upon by pillars of water equal

Fig. 5.

qual in height to the part *XA*, or *WB*, if the whole weight of the wooden Cube be greater then that of as much water as is equal to the immers'd part, it muſt neceſſarily ſinck lower, becauſe the ſubjacent part of the Surface (at *V*,) will be more charg'd then any of the Reſt. And, on the other ſide, if the Cube were lighter then as much water as that whoſe place the immers'd part takes up; it muſt by the greater preſſure of the water upon the other parts of the imaginary ſuperficies *XW*, then upon that contiguous to the wood, (as at *V*) be impell'd upward, til the preſſure of the whole wood upon the part it leans on, be of the ſame degree with that of the reſt of the water, upon the reſt of the ſuperficies: and conſequently be the ſame with the water, whoſe place the immers'd part of it takes up. The lightneſs of that immers'd part, in reſpect of ſo much water, being re-

compenc'd

compenc'd by the weight of the unimmers'd part , which is extant above the Superficies of the water. And we ſee, that when a piece of wood fals into water, though, by the impetus it acquires in falling, it paſſes through divers imaginary plains that lye beneath its due ſtation ; yet the greater preſſure, to which each of thoſe plains is expos'd in all its other parts, then in that which is contiguous to the Bottom of the wood, dos quickly impel it up again, till, after ſome emerſions and ſubſidings, it reſts at length in ſuch a poſition, as the newly explicated Hydroſtatical Theorem aſſignes it.

SCHOLIUM.

THis Ingenious Propoſition (about floating Bodys) is taught and prov'd after the manner of *Mathematicians*, by the moſt ſubtle *Archimedes* and his Commentators : and we have newly been endeavouring to manifeſt the Phyſical reaſon why it muſt be true. But *partly* becauſe the Propoſition ought to hold, not only in ſuch intire and homogeneous Bodyes as men exemplifie it in , (ſuch as a piece of wood, or a Lump of wax) but in all Bodyes, though of a concave figure, and made up of many Bodys of never ſo differing natures ; (and perhaps ſome of them joyn'd together only by their ſuperincumbency upon one another) and partly becauſe that a Truth, which is one of the main and uſefulleſt of the Hydroſtaticks, and may be of ſo much importance to Navigation, has no yet (that I know of) been attemtpted to be demonſtrated otherwiſe then upon Paper: it will not be amiſs, for the ſatisfaction of ſuch of thoſe whom it may concern, as are not vers'd in Mathematical *Demonſtrations*, to add an Experiment which I made to prove it *Mechanically*; as exactly as is neceſſary for the ſatisfaction of ſuch perſons.

After (then) having imploy'd ſeveral Veſſels, ſome of wood, ſome of Laton, and ſome of other materials, to compaſs what I deſir'd; we found glaſſes to be the moſt commodious we could procure. And therefore filling a large and deep glaſs to a convenient height with fair water, we plac'd in it another deeper glaſs, ſhap'd like a *Goblet* or Tumbler, that it might be the fitter for ſwimming; and having furniſh'd it firſt with *Ballaſt*, and then, for merryment ſake, with a wooden Deck, by which a tall Maſt, with a Sayle faſten'd to it, was kept upright; we fraughted with wood, and by degrees pour'd Sand into it, till we had made it ſinck juſt to the Tops of certaine conſpicuous marks, that we had faſten'd on the outſide of the Glaſs to oppoſite parts thereof. Then obſerving how high the water reach'd in the larger Glaſs, (which by reaſon of the Veſſels Tranſparency was eaſie to be ſeen) we carefully plac'd two or three markes in the ſame level with the Horizontal Surface of the water; and taking out the floating Veſſel, as it was, with all that belong'd to it, and wiping the outſide dry, we put it into a good paire of ſcales, and having found what it amounted to, we weigh'd in a competently large Viol (firſt counterpoiz'd apart) ſo much water, (to a graine, or thereabouts,) and pouring this water into the large Glaſs above mentioned, we found it to reach to the marks that we had faſtened to the outſide of the Glaſs, and conſequently to reach to the ſame height to which the weight of the floating glaſs, and all that was added to make it reſemble a Ship, had made it ariſe to. By which Experiment (wch we tried, as to the eſſential parts of it, with Veſſels of differing ſizes, ſhapes; and ladings too, as Wood, Stone, Quickſilver, *&c.*) it appears, that the floating Veſſel it ſelf, with all that was in it, or ſupported by it, was equal in weight to as much water as was equal in bulk to that part of the Veſſel which was under water, ſuppos'd to be cut off from the extant part of the ſame veſſel by a plain continuing the Horizontal Surface of the water: ſince the weight of the floating Veſſel, which rais'd up the water in the larger Veſſel to the greateſt height it attain'd, was the ſame with the weight of the water, which being pour'd into the larger veſſel (when the other was taken out) rais'd the water therein to the ſame height. We may alſo obtaine the ſame end, by a ſomewhat differing way, (which is the beſt way in caſe the Veſſels be too great) *viz.* to obſerve, firſt, by pouring in water out of a Bowle or Paile, or other Veſſel of known capacity, as often as is neceſſary to fill the great Veſſel, or Ciſtern, or Pond, to the Top, (or to any determinate height requir'd) and, next, letting out, or otherwiſe removing all that water, to put in its place the Veſſel, whoſe weight is to be found out. Thirdly, to let, or poure in, water till the Veſſel be afloat, and by its weight raiſe the External water to the height it had before: And laſtly, to examine how much this water, that was laſt pour'd in, falls ſhort in weight of the water that was in it at firſt, and afterwards remov'd. For this difference will give us the weight of as much water, as is æquiponderate to the whole floating Veſſel, whither ſmall or great, with all that it either carries or ſuſteins. The Hydroſtatical Theorem we have been conſidering, and the Experiments whereby we have endeavour'd to confirm, or illuſtrate it, may (*Mutatis mutandis*) be apply'd to a Ship with all her Ballaſt, Lading, Guns, and Company; it holding generally true, *That* (to expreſs the ſence of the Propoſition more briefly) *the weight of a floating Body, is equal to as much water, as its immers'd part takes up the room of.* Whence we might draw ſome Arguments in favour of the Learned *Stevinus*, (for whoſe ſake it partly was that I annexed this *Scholium*) who, if I miſ-remember not, does ſomewhere deduce as a Corollary from certain Hydroſtatical Propoſitions, That a whole Ship, and all that belongs to it, and leans upon it, preſſes no more nor leſs upon the Bottome it ſwims over, then as much water, as is equal in bulk to that part of the Ship which is beneath the Surface of the water.

See PARADOX *the ſixth.*

PARA-

PARADOX IV.

That in the Aſcenſion of water in Pumps, &c. there needs nothing to raiſe the Water, but a competent weight of an External Fluid.

THis Propoſition may be eaſily enough deduc'd from the already mention'd Experiments. But yet, for further illuſtration and proof, vve vvill add that vvhich follovvs.

Take a ſlender Glaſs-pipe, (ſuch as vvas us'd about the firſt Experiment) and ſuck into it about the height of an Inch of deeply tincted vvater; and, nimbly ſtopping the upper Orifice, immerſe the lovver part of the pipe into a Glaſs half fill'd vvith ſuch tincted vvater, till the Surface of the Liquor in the pipe be an inch (or as low as you would have it) beneath that of the External water. Then pouring on oyle of Turpentine till it ſwim 3 or 4 Inches, or as high as you pleaſe above the vvater; looſen gently your finger from the upper Orifice of the pipe, to give the incloſed Aire a little intercourſe vvith the External, and you ſhall ſee the tincted vvater in the pipe, to be impell'd up, not only higher then the Surface of the External vvater, but almoſt as high as that of the External oyl, through vvhich (it being tranſparent and colourleſs) the Red Liquor may be eaſily diſcern'd.

Novv in this caſe it can't be pretended, That the aſcent of the water in the pipe proceeds from Natures abhorrency of a *Vacuum*; ſince the pipe being full of aire, and its Orifice unſtopp'd, though the vvater ſhould not aſcend, no danger of a *Vacuum* vvould enſue; the aire and the vvater remaining contiguous as before.

The true Reaſon then of the aſcent of the water, in our caſe, is but this; That upon all the other parts of the Imaginary Superficies, that paſſes by the immers'd Orifice of the pipe, there is a preſſure partly of water, and partly of the oyle ſwimming upon that water, amounting to the preſſure of 4 or 5 inches of water; whereas upon that part of the ſame ſuperficies whereon the Liquor contain'd in the pipe leans, there is but the preſſure of one inch of water, ſo that the parts near the immers'd Orifice muſt neceſſarily be thruſt out of place by the other parts of water that are more preſs'd; till ſo much Liquor be impell'd up into the pipe as makes the preſſure on that part of the Imaginary Superficies, as great as that of the oyle and water on any other equal part of it: and then, by Vertue of the *Æquilibrium*,(often mention'd) the water will riſe no further; and, by vertue of the ſame *Æquilibrium*, it will reſt a little beneath the Surface of the External oyle, becauſe this laſt nam'd Liquor is leſſe heavy, bulk for bulk, then water.

And by this we may be aſſiſted to give a reaſon of the Aſcenſion of water in ordinary ſucking Pumps. For as the oyle of Turpentine, though a lighter Liquor then water, and not mingleable with it, does, by leaning upon the Surface of the External water, preſs up the water within the pipe, to a far greater height then that of the External water it ſelf: ſo the Aire, which, though a far lighter Liquor then oyle of Turpentine, reaches I know not how many Miles high, leaning upon the Surface of the water in a Well, would preſs it up into the Cylindrical Cavity of the Pump, much higher then the External water it ſelf reaches in the Well, if it were not hinder'd.

Now that which hinders it in the

Pump, is either the Sucker, which fences the water in the Pump from the preſſure of the External aire, or that preſſure it ſelf. And therefore, all that the drawing up of the Sucker needs to do, is, to free the water in the Pipe from the impediment to its Aſcent, which was given it by the Suckers leaning on it, or the pillar of the Atmoſphæres being incumbent on it;as in our Experiment, the ſides of the pipe do ſufficiently protect the water in the pipe from any preſſure of the External oyle, that may oppoſe its aſcent.

And laſtly, as the water in our pipe was impell'd up ſo high, and no higher, that the Cylinder of water in the pipe was juſt able to ballance the preſſure of the water and oyle without the pipe; ſo in Pumps, the water does riſe but to a certain height, as about 33 or 34 foot: and though you pump never ſo long, it will be rais'd no higher; becauſe at that height the preſſure of the water in the Pump, upon that part of the imaginary Superficies that paſſes by the lower Orifice of it, is the ſame with the preſſure which other parts of that imaginary ſuperficies ſuſtaine from as much of the External water, and of the Atmoſphære, as come to lean upon it.

That there may be caſes wherein water may be rais'd by ſuction, not upon the Account of the weight of the aire, but of its ſpring, I have elſewhere ſhovvn; and having likevviſe in other places, endeavour'd to explicate more particularly the aſcenſion of vvater in Pumps; vvhat has been ſaid already may ſuffice to be ſaid in this place, where 'tis ſufficient for me to have ſhovvn, That vvhither or no the Aſcenſion of water *may have* other cauſes, yet in the caſes propos'd, it *needs* no more then the competent vveight of an External Fluid, as is the Aire; vvhoſe not being devoid of gravity, the Cogency of our Experiments has brought even our Adverſaries to grant us.

For confirmation of this, I will here add, becauſe it now comes into my mind, (what might perhaps be elſewhere ſomewhat more properly mention'd) an Experiment that I did but lightly glance at in the Explication of the firſt, and the *Scholium* of the ſecond Paradox.

In order to this I muſt advertiſe, That, whereas I there took notice, that ſome Ingenious men had complain'd, that, contrary to the Experiment propos'd by *Monſieur Paſchall*, they were not at all able to keep *Mercury* ſuſpended in Tubes, however very ſlender, though the lower end were deeply immers'd in water, if both their ends were open: The Reaſons of my doubting, whether our Ingenious Author had ever made or ſeen the Experiment, were, not only that it had been unſuccesfully tryed, and ſeem'd to me unlikely to ſucceed in Tubes more ſlender then his appear'd; but becauſe the Impetus, which falling quickſilver gains by the acceleration of motion it acquires in its deſcent, muſt in all probability be great enough to make it all run out at the bottom of a Tube, open at both ends, and fill'd with ſo ponderous a Liquor, though the Tube were very much ſhorter then that propos'd by *Monſieur Paſchall*.

This advertiſement I premiſe to intimate, that, notwithſtanding the hopeleſſneſs of the Experiment, as it had been propos'd and tried, I might have reaſon not to think it impoſſible to perform, by another way, the main thing deſir'd; which was to keep Quickſilver ſuſpended in a Tube, open at both ends, by the reſiſtance of the ſubjacent water. For by the Expedient I am going to propoſe, I have been able to do it, even with a Liquor much lighter then water.

Finding then, that even a very ſhort Cylinder of ſo ponderous a fluid, as *Mercury*, would, if it were once in falling, deſcend with an impetus not eaſy to be reſiſted by the ſubjacent Liquor, I thought upon the following Expedient to prevent this inconvenience. I took a ſlender pipe, the Diameter of whoſe Cavity was little above the ſixth part of an Inch, and having ſuck'd in at the lower end of it ſomewhat leſſe then half an inch of Quickſilver, and nimbly ſtopp'd the upper Orifice with my finger; I thruſt the Quickſilver into a deep glaſs of oyle of Turpentine, with a care not to unſtop the upper Orifice, till the ſmall Cylinder of quickſilver was 18 or 20 times its depth beneath the Surface of the oyle. For by this means, when I unſtopp'd the pipe, the Quickſilver needed not (as otherwiſe it would) begin to fall, as having a longer Cylinder then was requiſite to make an *Æquilibrium* with the other fluid. For by our Expedient the preſſure of the oyle was already full as great, if not greater, againſt the lower part of the Mercurial Cylinder, as that which the weight of ſo ſhort a Cylinder could exerciſe upon the contiguous and ſubjacent oyle. And accordingly, upon the removal of my finger, the Quickſilver did not run out, but remain ſuſpended in the lower part of the pipe. And as, if I rais'd it towards the Superficies of the oyle, the Mercury would drop out for want of its wonted Counterpoize; ſo, if I thruſt the pipe deeper into the oyle, the increas'd preſſure of the oyle would proportionably impell up the Mercury towards the higher parts of the pipe, which being again a little, and but a little, rais'd, the Quickſilver would fall down a little nearer the bottom of the pipe: and ſo, with a not unpleaſant ſpectacle, the ponderous Body of quickſilver was made ſometimes to riſe, and ſometimes to fall; but ſtill to float upon the Surface of a Liquor, lighter then common Spirit of Wine it ſelf.

But, beſides that the Experiment, if the maker of it be not very careful, may eaſily enough miſcarry, the divertiſement it gives ſeldome proves laſting; the oyle of Turpentine after a while inſinuating it ſelf betwixt the ſides of the pipe, and thoſe of ſo ſhort a Cylinder of Mercury, and thereby diſordering all. And therefore, though I here mention this Experiment, as I tryed it in oyle of Turpentine; becauſe that is the Liquor I make uſe of all along theſe Paradoxes; and becauſe alſo I would ſhew that a lighter fluid then water, (and therefore why not aire, if its height be greatly enough increas'd?) may by its weight and preſſure, either keep the Mercury ſuſpended in pipes, or even raise it in them: Yet I found water (wherewith I fill'd tall glaſſes) a fitter Liquor then oyle for the Experiment; in which though I ſought, and found ſome other Phænomena, yet becauſe they more properly belong to another place, I ſhall leave them unmention'd in this.

And ſince Experience ſhews us, that a Cylinder of Mercury, of about 30 Inches high, is æquiponderant to a Cylinder of water of about 33 or 34 foot high; its very eaſie to conclude, That the weight of the External aire, which is able to raiſe and keep ſuſpended 33 or 34 foot of water in a Pump, may do the like to 29 or 30 Inches of Quickſilver in the Torricellian Experiment.

PARADOX V.

That the preſſure af an External Fluid is able to keep an Heterogeneous Liquor ſuſpended at the ſame height in ſeveral Pipes, though thoſe Pipes be of very different Diameters.

THE contrary of this Propoſition is ſo confidently aſſerted and believed, by thoſe Mathematicians, and others, that favour the Doctrine of the Schools; That this perſwaſion of theirs ſeems to be the chief thing, that has hinderd men from acknowledging, that the Quickſilver in the Torricellian Experiment may be kept ſuſpended by the Counterpoize of the external aire. And a famous writer, that has lately treated, as well of the Hydroſtaticks, as of the Phænomena of the Torricellian Experiment, dos rely ſo much upon the falſehood of our Paradox, That, laying aſide all other Arguments, he contents himſelf to confute his Adverſaries with one Demonſtration (as he calls it) grounded on the quite contrary of what we here aſſert. For his Objection runs to this ſence. That if it were the preſſure of the External Aire, that kept the Quickſilver ſuſpended in the newly mention'd experiment, the height would not (as Experience ſhews it is) be the ſame in all Cylindrical pipes, though of very differing Bores. For, ſuppoſing the height of the Mercurial Cylinder, in a Tube of half an Inch Diameter, to be 29 Inches; 'tis plain, that a Mercurial Cylinder of the ſame height, and three Inches in Diameter, muſt weigh divers times as much as the former; and therefore the preſſure of the External aire, being but one and the ſame, if it be a juſt Counterpoize to the greater Cylinder, it cannot be ſo to the leſs; and if it be able to keep the one ſuſpended at 29 Inches it muſt be able to keep the other ſuſpended at a far greater height, which yet is contrary to experience. And indeed this Objection is ſo ſpecious, That, though I elſewhere have already anſwer'd it, both by reaſon and Experience, as far forth as it concerns the Torricellian Experiment; Yet, to ſhew the miſtake on which it is grounded, it may be very well worth while to make out our propoſed Paradox, (as that whoſe truth will ſufficiently diſprove that errour) by ſhewing both *that* the Aſſertion is true, and *why* it muſt be ſo.

Provide then a more then ordinarily wide mouth'd Glaſs, cleer, and of a Convenient depth; into which having put a con- *Fig.* 6.

venient quantity of water, deeply ting'd with Brazil or ſome other Pigment, fit to the Orifice a broad but thin Cork, in which,by burning or cutting, make divers round holes of very differing wideneſſes; into each of which you may thruſt a glaſs Cylinder, open at both ends, and of a ſize fit for the hole that is to receive it; that ſo the ſeveral pipes may be imbrac'd by theſe ſeveral holes; And, as neare as you can, make them parallel to one another, and perpendicular to the ſuperficies of the water, into which they are to be immers'd. But we muſt not forget, that, beſides theſe holes, there is an aperture to be made in the ſame Corke (it matters not much of what figure or whereabouts) to receive the ſlender end of a glaſs Funnel; by which oyl may be convey'd into the veſſel,when it is ſtopp'd with the Cork. And in the ſlender part of this Funnel we uſe to put ſome Cotton-week, to break the violence of the oyl that is to be pour'd in, which might elſe diſorder the Experiment. All this being thus provided, and the Cork (furniſh'd with its pipes) being fitted to the Orifice of the Veſſel; if at the Funnel you pour in oyl of Turpentine, and place the Glaſs betwixt your eye and the Light; you may, through that tranſparent Liquor, perceive the Tincted water, to be impell'd up into all the pipes, and to riſe uniformly in them. And, when this tincted Liquor has attain'd to the height of two or three, or more Inches, above the lowermoſt Surface of the External oyl; if you remove the Funnel, (which yet you need not do, unleſs there be yet oyl in it,) you may plainly perceive the water to reach as high, in one of the ſmaller pipes, as in another three or four times as great; and yet the water in the ſeveral pipes (as 'tis evident) is ſuſtain'd, at that height above the level of the other water, by the preſſure or counterpoize of the external oyle; which therefore being lighter *in ſpecie* then water, will have its Surface ſomewhat higher without the pipes, then that of the Tincted water within them. And if by the Aperture, that receives the Funnel, you immerſe, almoſt to the Bottom of the oyle, the ſhorter leg of a ſlender glaſs Syphon, at whoſe longer Leg you procure by Suction the oyle to run out; you ſhall perceive, That, according as the depth and preſſure of the External fluid decreaſes, ſo the water in the pipe will ſubſide; and that uniformly, as well in the leſſer as in the greater pipes.

The Reaſon of this is not difficult to be render'd, by the Doctrine already deliver'd. For ſuppoſe *E F* to be the Surface of the water, both within and without the pipes, before any oyle was poured on it: if we then ſuppoſe the oyle to be poured in through the Funnel, its lightneſs in reſpect of water, wherewith it will not mingle, will keep it from getting into the cavity of the pipes *L*, *M*, *N*; and therefore ſpreading it ſelf on the outſide of them above, it muſt neceſſarily, by its gravity, preſs down the Superficies of the external water, and impell up that liquor into the cavities of the pipes. And if we ſuppoſe the pouring on of the oyl to be continued till the uppermoſt ſurface of the oyl be raiſed to *G H*, and that of the external water depreſs'd to *I K*, (or thereabouts,) an imaginary plain paſſing along the lower Orifices of the pipes; I ſay, the tincted waters in the pipes ought to have their uppermoſt Surfaces in the ſame level, notwithſtanding the great inequality of their Bores. For that part of the Surface

IK, which is comprehended within the Circular Orifice of the greateſt pipe *L*, is no more charged by the incumbent water, then any other part, equal to that Circle of the ſame Imaginary Superficies, is by the water or oyle incumbent on it; (and conſequently, no more then the part comprehended within the circle of the ſmall pipe *N*, is by the water contain'd in that ſmall pipe;) the external oyle having as much a greater height upon the Superficies *I K*, then the water within the pipe, as is requiſite to make the two Liquors Counter-ballance each other, notwithſtanding the difference of their ſpecifick Gravities. And though the pipe *L* were twice as bigg, it would Charge the ſubjacent plain *I K* no more, then the preſſure of the oyle on the other parts of the ſame imaginary Surface is able to reſiſt. And yet this preſſure of the External oyle ought not

to

to be able to raiſe the water in the ſlender pipe *N*, higher then the Surface *Q* in the ſame Level with the Surface *O*. For, if the water were higher in the ſmall pipe; being a heavier Liquor then oyle, it muſt preſs upon that part of the Surface *I* K, it leans on, with greater force then the external oyle upon the other parts of the ſame plain *I* K; and therefore with greater force then the weight of the External oyle could reſiſt. And conſequently, the water in the ſlender pipe muſt ſubſide, till its Surface be inferiour to that of the External oyle; ſince, till then, the difference of their ſpecifick gravities cannot permit them to reſt in an *Æquilibrium*. To be ſhort; It is all one, to the reſiſtance of the external oyl, how wide the Cylinder is that it ſupports in the pipe; provided the height of it be not greater in reſpect of the height of the oyl, then the difference of the reſpective Gravities of thoſe two Liquors requires. For, ſo long the preſſure of the Cylinder of water will be no greater, on that part of the Imaginary Superficies which it leans upon, then the preſſure of the external oyle will be on all the other parts of the ſame Superficies; and conſequently, neither the one, nor the other of thoſe Liquors will ſubſide, but they will both reſt in an *Æquilibrium.*

But here it will not be amiſs to note, Firſt, that it is not neceſſary that the Glaſs Cylinders *L*, *M*, *N*, ſhould be all of the ſame length; ſince, the lower Orifice being open, the water will riſe to the ſame height within them, whether the parts immers'd under the water be exactly of the ſame length or no.

And Secondly, That throughout all this Diſcourſe, and particularly in the

Explication of this Paradox, we ſuppoſe, either that the ſlendereſt pipes, that are imploy'd about theſe Experiments, are of a moderate ſize, and not exceeding ſmall; Or that, in caſe they be very ſmall, allowance be made in ſuch pipes for this property, That water will riſe in them to a greater height, then can be attributed to the bare Counterpoize of either the water or the oyle, that impels it upwards and keeps it ſuſpended. But this difference is of ſo little moment in our preſent Inquiries, That we may ſafely neglect it, (as hereafter we mean to do) now we have taken this notice of it for prevention of miſtakes.

PARADOX VI.

If a Body be plac'd under water, with its uppermoſt Surface parallel to the Horizon; how much water ſoever there may be on this or that ſide above the Body, the direct preſſure ſuſtain'd by the Body (for we now conſider not the Lateral nor the recoyling preſſure, to which the Body may be expos'd if quite environ'd with water) is no more then that of a Columne of water, having the Horizontal ſuperficies of the Body for its Baſis, and the perpendicular depth of the water for its height.

And ſo likewiſe,

If the water that leans upon the Body be contain'd in pipes open at both ends; the preſſure of the water is to be eſtima-

ted

ted by the weight of a pillar of water, whoſe Baſis is equal to the lower Orifice of the pipe,(which we ſuppoſe to be parallel to the Horizon) and its height equal to a perpendicular reaching thence to the top of the water; though the pipe be much inclin'd towards the Horizon, or though it be irregularly ſhap'd, and much broader in ſome parts, then the ſaid Orifice.

S*Tevinus*, in the tenth Propoſition of his Hydroſtatical Elements, having propos'd in more general termes, the former part of our Paradox, annexes to it a Demonſtration to this purpoſe.

Having firſt ſuppoſed *A B C D*, to be a ſolid Rectangular figure of water, whoſe Baſis *E F* is parallel to the Horizon, and whoſe height *G E* is a perpendicular let fall from the uppermoſt Surface of the water to the lowermoſt; His Demonſtration is this;

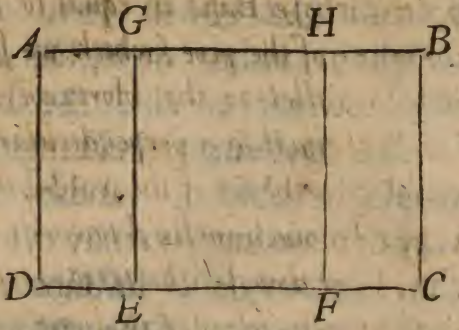

Fig. 7.

If the Bottom *E F* be charged with a greater weight then that of the water *G H F E*, that ſurpluſage muſt come from the adjoyning water; therefore, if it be poſſible, let it be from the water *A GE D*,& *H B C F*;which granted, the Bottom *D E* will likewiſe have a greater weight incumbent on it, upon the ſcore of the neigbouring water *G H F E*, then that of the water *A G E D*. And, the reaſon being the ſame in all the three caſes, the Baſis *F C* muſt ſuſteine a greater weight, then that of the water *HBCF*. And therefore the whole bottom *DC*, will have a greater weight incumbent on it, then that of the whole water *A B C D*; which yet (*A B C D* being a rectangular Body) would be abſurd. And by the ſame way of reaſoning you may evince, That the Bottom *E F* ſuſtains no leſs a weight, then that of the water *G H F E*. And ſo, ſince it ſuſtains neither a greater weight, nor a leſs, it muſt ſuſtein juſt as much weight as the Columne of water *G H F E*.

This Demonſtration of the Learned *Stevinus* may well enough be admitted by a Naturaliſt (though, according to ſome Hypotheſes touching the Cauſe and Nature of Gravity, it may faile of Mathematical exactneſs;) and by it may be confirm'd the firſt part of our propos'd Paradox. And ſome things annexed by *Stevinus* to this Demonſtration, may be alſo apply'd to countenance the ſecond. But becauſe this is one of the nobleſt and uſefulleſt Subjects of the Hydroſtaticks; we think it worth while to illuſtrate, after our manner, each of the two parts of our Paradox by a ſenſible Experiment.

Firſt then, Take a ſlender Glaſs pipe, of an even Bore, turn'd up at one end like the annexed Syphon. Into this Syphon ſuck oyl of Turpentine till the Liquor have fill'd the ſhorter leg, and be rais'd 2 or 3 Inches in the longer. Then nimbly ſtopping the upper Orifice with your finger, thruſt the lower part of the Syphon ſo farre into a deep Glaſs full of water, That the Surface of the oyle in the longer leg of the pipe, may be but a little higher then that of the External water; and, upon the removal of your finger, you will find the Surface of the oyle to vary but little, or not at all, its former Station. *Fig. 8.*

And as, if you then thruſt the pipe a *little deeper, you* will ſee the oyle in *the ſhorter* leg to begin to be depreſs'd; *ſo,* if afterwards you gently raiſe the pipe toward the top of the water, you *ſhall* ſee the oyle not only regain its former ſtation, but flow out by degrees in drops that will emerge to the Top of the water. Now, ſince the water was able, at firſt, to keep the oyl, in the longer leg of the pipe, ſuſpended no higher, then it would have been kept by a Cylinder of water equal to the Orifice of the ſhorter leg of the pipe, and reaching directly thence to the Top of the water; (as may be eaſily tried, by making a Syphon, where the ſhorter leg may be long enough to contain ſuch a Cylinder of water to counterpoize the oyl in the longer;) & ſince, when once, by the raiſing of the pipe, the height of the incumbent water was leſſen'd, the oyle did more then Counter-ballance it; (as appears by its flowing out of the Syphon;) we may well conclude; That, though there were in the Veſſel a great deal of water, higher then the immers'd Orifice of the Syphon, (and it would be all one, though the Syphon were plac'd at the ſame depth in a pond or lake;) yet, of all that water, no more did gravitate upon the Orifice, then that which was plac'd directly over it; which was ſuch a pillar of water, as the Paradox deſcribes.

And, by the way, we may hence learn; That though water be not included in pipes, yet it may preſs as regularly upon a ſubjacent Body, as if it were. And therefore we may well enough conceive a pillar of water, in the free water it ſelf, where there is nothing on any ſide, but the contiguous water, to bound the imaginary pillar.

But I had forgot to add, That the firſt part of our Paradox will hold, not only when the water, ſuperior to the Body it preſſes upon, is free; but alſo, when it is included in Veſſels of never ſo (ſeemingly) diſadvantageous a ſhape. For, if you ſo frame the ſhorter leg of a Syphon, that it may expand its ſelf into a funnel, like that of *Fig.*6. employ'd about the proof of the foregoing (fifth) Paradox; (for which purpoſe the legs muſt be at a pretty diſtance from each other:) though you fill that Funnel with water, the oyle in the longer and ſlender leg of the Syphon will be able to reſiſt the preſſure of all the water, notwithſtanding the breadth of the upper part of the funnel. So that, ev'n in this caſe alſo, the Surface of the oyle in the longer leg, will be but a little higher then that of the water in the funnel.

For further Confirmation of this;

we cauſ'd to be made a Syphon, ſo ſhap'd, that one of the legs (which were parallel, and of the ſame Bore,) had in the midſt of it a Sphære of Glaſs, ſave that it communicated with the upper and lower parts of the ſame leg.

In the uniform leg of the Syphon, we put a convenient quantity of oyle of Turpentine, and into the other, as much water as fill'd not only the lower part of it, but the Globular part too. And yet we did not find, that all this water was able to keep up the oyle in the uniform leg, at a greater height then if the leg that contain'd the water had been uniform too; as much of the water in the Globe, as was not directly over the lower Orifice of it, being ſupported by the lateral parts (if I may ſo call them) of the ſame Globe. And, if that leg were, inſtead of water, fill'd with oyle, and the uniform leg with water ; notwithſtanding the far greater quantity of oyl , that was neceſſary to fill that leg, whereof the hollow ſphære was but a part ; the water in the uniform leg would not be kept up, ſo much as to the ſame height with the oyle in the miſhapen leg.

But to make this matter yet the more clear, we caus'd a Syphon to be made of the Figure expreſs'd in the adjoyning Scheme ; into which having poured a convenient quantity of Mercury, till it reach'd in the ſhorter leg *C D*, almoſt to the bottom of the Globulous part *E*, and in the longer leg *A B*, to an equal height: We afterwards, poured a ſufficient quantity of water into the ſaid longer leg *A B*, which drove away the Quickſilver, and impell'd it up in the ſhorter leg till it had half, or more then half, fill'd the Cavity of the Globular part *E*; (which

Figur. 9.

yet we did not wholly fill with Quickſilver, becauſe the Tube *A B* was not long enough for that purpoſe;) and then we obſerv'd, that, notwithſtanding the great weight of (that Body, which is of all Bodies, ſave one, the moſt Ponderous) Quickſilver, which was contain'd in the lower part of the ſame leg of the Syphon, the ſurface of the Quickſilver *H G*, was impell'd up as high by the water in the Leg *A B*, as the diſparity of the ſpecifick weights of thoſe two Liquors (whereof one is about 14 times as heavy as the other) did require: So that it appear'd not, that, for all the great weight of Quickſilver, contain'd in the Globulous Cavity *E*, there preſs'd any more upon the ſlender and ſubjacent part *E C* of that leg, then as much as was plac'd directly over the lower Orifice of the ſaid Cavity *E*. So that the other, and lateral parts of that Mercury, being

ing ſupported by the concave ſides of the Glaſs, whereunto they were contiguous, the water in the leg *A B*, appear'd not any more preſs'd by the quickſilver, then if the leg *C D* had been, as well as the other, of an uniform bigneſs; and, by this means, if we had made the hollow Globe of a large Diameter, a ſmall quantity of water, poured into the leg *A B*, might have been able to raiſe a quantity of quickſilver exceedingly much heavier then it ſelf. But then ſo little water can raiſe the quickſilver, in ſo broad a pipe, but to an inconſiderable height.

To make out the ſecond part of our Paradox by an Experiment, we took three Glaſs-pipes; the one made like a Bolt-head, with a round Ball and two oppoſite Stemms; the other was an irregular pipe, blown with an Elbow, wherewith it made an Angle; and the third

Fig. 10.

was as irregularly ſhap'd, as I could get it blown; being in ſome places much broader, and in ſome much narrower then the lower Orifice of it. And theſe two laſt nam'd pipes had their upper ends ſo inſerted into holes, made fit for them in a broad piece of Cork; that, when they were immers'd, they made not right Angles, but very oblique ones, with the Horizontal Surface of the Liquor. The other Glaſs likewiſe, which conſiſted of a great Bubble, and two oppoſite pipes, was faſtened to the ſame Cork, which having before hand been made fit for a wide mouth'd glaſs of a good depth, and half fill'd with water, was thruſt as a ſtopple into the mouth of the ſaid glaſs, ſo that the water aſcended a pretty way into each of the three pipes by their lower Orifices, which as well as the upper we left open; Then a good quantity of oyle of Turpentine being pour'd into the ſame Veſſel, through a funnel, the water was by the incumbent oyle impell'd up to the height of 2 or 3 Inches in each of the three pipes. Which argues, that, notwithſtanding their being ſo unequal in bigneſs, and ſo irregular in ſhape,(inſomuch that we gueſs'd one of them was 10 or 12 times greater in one part, then in another, or then it was even at the Orifice) the water, contain'd in each of them,preſs'd upon its lower Orifice *no more* (I do not add, nor no leſs) then it would have done if it had been a Cylinder, having the Orifice for its Baſis, and the perpendicular depth of the water and oyle above, for its height. For in caſe each of the pipes had contein'd but ſuch a Cylinder of water, that water would nevertheleſs have had its uppermoſt Superficies at the ſame height: and on the other ſide, it would have been impell'd up beyond it, if its weight did not as ſtrongly endeavour to depreſs the immediately ſubjacent water, as the preſſure of the External fluids endeavour'd to impel it up.

And ſince the height of the water was about the ſame in the ſeveral pipes, though two of them, being very much inclin'd, contain'd much more water then if they were erected: yet by the ſame way of reaſoning we may gather, That the imaginary plain, paſſing by the immers'd Orifice of either of theſe inclining pipes, ſuſtain'd no more of preſſure, then it would have done from a ſhorter Cylinder of water if erected. And indeed, in all theſe caſes, where a pipe either is broader in other places then at its lower Orifice, or inclin'd any way towards the Horizon, the weight of the contain'd Liquor is not all ſupported by the Liquor or the Body contiguous to the lower Orifice, but partly by the ſides of the pipe it ſelf. And therefore if, when in a ſlender pipe you have brought a parcel of oyl of Turpentine to be in an *Equilibrium* with the External water, as in the Experiment belonging to the firſt Paradox; If, I ſay, when this is done, you incline the pipe towards the ſides of the Glaſs, You may indeed obſerve the Surface of the oyle in the pipe to be, as before, a little higher then that of the water without it: But you ſhall likewiſe ſee, That, though the Orifice of the pipe were not thruſt deeper into the water, yet there will be a pretty deal of water got up into the pipe; becauſe the oyle not leaning now upon the water only, as it did before, but partly upon the water, and partly upon the pipe, its preſſure upon the ſubjacent water is conſiderably leſſen'd; and thereby the external water, whoſe preſſure is not diminiſh'd too, is able to impel up the oyl, and intrude for a little way into the pipe. But if you re-erect the pipe, the preſſure of the oyle being then again exerted upon the ſubjacent water, it will be able to depreſs, and drive it again out of the Cavity of the pipe.

And to this agrees very well what we further try'd as follows: We cauſed 3 pipes to be blown (ſhap'd as the adjoyning Figures;) one having in it divers acute Angles; the other being of a winding form, like a *ſcrue* or *worm* of the Limbeck; and the third very irregularly crooked; and yet each of theſe pipes having all its crooked parts, and ſome of its ſtreight & erected parts, fil'd with oyl of Turpentine; being thruſt to a convenient depth under water and unſtopp'd there, (after the manner already often declar'd) we found, that, according to our Paradox, the ſurface of the oyle in the pipe was higher than that of the water without it, as much as it would have been in caſe the pipe had been ſtreight, (as we try'd by placing by the crookedeſt of them a ſtreight pipe with oyle in it) though the quantity *Fig.* 11.

of

of the oyle, in one of theſe pipes, were perhaps three times as much as would have ſuffic'd, if the pipe had been ſtrait: So that this ſurpluſage of oyle did not preſs upon the ſubjacent water, (for if it had done ſo, the oyle would have run out of the pipe.) And I remember, that lifting up as much of one of theſe crooked pipes, as I thought fit, ſomewhat above the Surface of the water; when the Superficies of the oyle in the pipe was not above half an inch higher then that of the water without it, I eſtimated that the crooked pillar of oyl, contain'd in that part of the *pipe* which was above the Surface of the water, was about 7 or 8 Inches long. So true it is, that the preſſure of Liquors, contain'd in pipes, muſt be computed by the perpendicular that meaſures their height, what ever be their length or bigneſs.

SCHOLIUM.

THE Learned *Stevinus*, having demonſtrated the Propoſition we lately mention'd out of him, ſubjoyns divers conſectaries of which the truth hath been thought more queſtionable, then that of the Theorem it ſelf. And therefore he thought fit to add a kind of Appendix to make good a Paradox, which ſeems to amount to this. That If, in the Cover of a large Cylindrical Box, exactly cloſed, there be perpendicularly erected a Cylindrical Pipe open at both ends, and reaching to the Cavity of the Box; this Inſtrument being fill'd with water, the circular Baſis of it will ſuſteine a preſſure, equal to that of the breadth of the *Baſis* and height of the Pipe.

I choſe thus to expreſs this Theorem, (which might be, according to *Stevinus*, propos'd in more general terms,) becauſe this way of expreſſing it will beſt ſuit with the ſubſequent Experiment, and may conſequently facilitate the ununderſtanding of the Paradox. But though the Learned *Stevinus*'s aims were to be commended; who finding this Propoſition doubted, ſeems to have had a great mind to give an Experimental Demonſtration of it, and therefore propoſes no leſs then five pragmatical Examples (as he calls them) to make out the truth of what he aſſerts; yet in this he hath been ſomewhat unhappy, that that Experiment, which alone (for ought I can find) has been try'd of all the five, is rejected as incompetent, by thoſe that profeſs to have purpoſely made tryal of it. And indeed, by reaſon of the difficulty of bringing them to a practical examen, I have ſomewhat doubted whether or no this uſeful writer did ever make all thoſe Tryals himſelf; rather then ſet down the events, he ſuppos'd they muſt needs have; as preſuming his conjectures rightly deduc'd from a Demonſtrative Truth. Wherefore though another of the Experiments, he propoſes, be not free from difficulty, yet having, by the help of an Expedient, made it practicable, we are induc'd by its plainneſs and clearneſs to prefer it to what elſe he propoſes to the ſame purpoſe.

See Fig.12.

We provided then a veſſel of Laton, of the figure expreſs'd in the Scheme, and furniſh'd it with a looſe Bottom *C D*, made of a flat piece of wood cover'd with a ſoft Bladder and greas'd on the lower ſide neer the edges, that leaning on the rim of wood *GH*, contiguous every where to the inſide of the Laton, it might be eaſily lifted from off this Rim; and yet lye ſo cloſe upon it, that the water ſhould not be able to get out between them: And to the midſt of this looſe bottom was faſtned a long ſtring, of a good ſtrength, for the uſe hereafter to be declared. The Inſtrument thus fitted, the water was poured in apace at the Top *A B*, which, by its weight preſſing the falſe Bottom *C D* againſt the ſubjacent Rim *GH*, contributed to make the Veſſel the more tight, and to hinder its own paſſing. The Veſſel being fill'd with water we took the forementioned ſtring, one of whoſe ends was faſtned to *I*, the middle part of the looſe Bottom; and, tying the other end K to the extremity of the Beam of a good pair of Scales, we put weights one after another into the oppoſite ſcale, till at length thoſe weights lifted up the falſe Botom *C D* from the Rim G H; and, conſequently, lifted up the Incumbent water; which preſently after ran down between them. And having formerly, before we poured in any water, try'd what water would ſuffice to raiſe the Bottom *C D*, when there was nothing but its own proper weight that was to be ſurmounted; we found, by deducting that weight from the weight in the ſcale, and comparing the Reſidue with the weight of as much water, as the cavity of the broad, but very ſhallow Cylinder *B E C H G D F* would have alone (if there had been no water in the pipe *A I*) amounted to; we found, I ſay, by comparing theſe particulars, that the preſſure upon *C D* was by ſo very great odds more, then could have been attributed to the weight of ſo little water, as the Inſtrument pipe and all contain'd, in caſe the water had been in an uniform Cylinder, and conſequently a very ſhallow one, of a Baſis as large as that of our Inſtrument, That we could not but look upon the ſucceſs, as that, which though it did not anſwer what the reading of *Stevinus* might make a man expect; yet may deſerve to be further proſecuted, that whether or no the Paradox of *Stevinus* (which not only ſome others, but the Learned Dr. *Wallis* himſelf queſtion) wil hold; the Inquiry he has ſtarted, may be ſo perſued, as to occaſion ſome improvement of this part of Hydroſtaticks: where, to define things with certainty, will perhaps be found a difficulter Task then at firſt glance one would think; both becauſe divers ſpeculative things muſt be taken into conſideration, whoſe Theory has not perhaps yet been clear'd, and becauſe of the difficulty that will be found in practice by them that ſhall go about to make *Stevinus*'s Experiments, or others of that ſort with all requiſite Accurateneſs: As indeed, it is far eaſier to propoſe Experiments, which would in likelyhood prove what we intend, in caſe they could be made, then to propoſe practicable Expedients how they may be made.

PARA-

PARADOX VII.

That a Body immers'd in a Fluid, ſuſtains a lateral preſſure from the Fluid; and that increas'd, as the depth of the immers'd Body, beneath the Surface of the Fluid, increaſeth.

THough I ſhall not wonder if this propoſition ſeems ſtrange enough to moſt Readers: yet I think I could make it out by ſeveral wayes, and particularly by one that is plain and eaſie, being but that which follows.

Take then a ſlender Glaſs pipe (like that imployd about the firſt Experiment;) and cauſe it to be bent within two or three Inches of one end, ſo that the longer and the *Fig.* 13.

ſhorter legs, *E F* and *F G*, may make, as near as can be, a right Angle at *F*; then dipping the Orifice of the ſhorter leg *F G* in oyle of Turpentine, ſuck into the Syphon (if I may ſo call it) as much of the Liquor, as will fill the ſhorter leg, and reach two or three Inches high in the longer; then, nimbly ſtopping the upper Orifice with your finger, immerſe the lower part of the Glaſs under water, in ſuch manner as that the longer leg *EF* may make, as to ſenſe, right Angles with (*A B*) the Horizontal Surface of the water, and the ſhorter leg *F G* may be ſo far depreſs'd under that Surface, That *I* K, the Superficies of the oyle in the longer leg, be but a little higher then *A B*, that of the external water. Then, removing your finger, you may obſerve, That the oyle in the Syphon will continue (with little or no change) in its former ſtation. By which it appears that there is a lateral preſſure of the water againſt the oyle contiguous to *G*, the Orifice of the ſhorter leg of the pipe, ſince it is only that preſſure that hinders the efflux of the oyl at that Orifice, notwithſtanding the preſſure of the perpendicular Cylinder of oyle that would drive it out.

And that this preſſure of the perpendicular Cylinder doth really urge the oyle in the ſhorter leg to flow out; you may learn by ſlowly lifting the Syphon (without changing its former poſture) towards the Surface of the water. For as the lower leg comes nearer and nearer to that Surface, (to which, as I newly intimated, it is ſtill to be kept parallel) the oyle in the Horizontal leg will be driven out in drops, by the preſſure of the other oyle in the perpendicular leg.

That likewiſe before you begin to raiſe the Syphon, the lateral preſſure of the water againſt the lower Orifice of it is, at leaſt in ſuch Experiments, near about the ſame with what would be the perpendicular preſſure of a Cylinder of water, reaching from the ſame Orifice *G* (or ſome part of it) to the top of the water, may be gather'd from hence, That the Surface of the oyle in the longer leg will be a litle higher then that of the external water, as (by reaſon of the often mention'd comparative levity of the oyl) it would be, if we ſuppoſe, That a pipe of Glaſs of the ſame bore, and reaching to the top of the water, being fitted to the Orifice of the Horizontal Leg (as in the annex'd figure the Cylinder, *G H*) were fill'd with water.

And, to make out the latter part of our propoſition, we need add no more, then that, if you plunge the Syphon deeper into the water, you ſhall find the oyle, by the Lateral preſſure of the water, driven by degrees quite out of the ſhorter leg into the longer: and if you thruſt it yet deeper, you may obſerve that the longer leg will admit a Cylinder of water, upon which that of oyle will ſwim; the whole oyle alone being unable to counterballance the lateral preſſure of the water at ſo great a depth.

By which laſt circumſtance, it appears, that water has alſo a lateral preſſure againſt water it ſelf, and that increas'd according to its depth; ſince otherwiſe the external water could not impel that in the Horizontal leg of the Syphon, into the perpendicular leg, though to doe ſo, it muſt ſurmount the weight or reſiſtance of the whole cylinder of oyl, that muſt be hereby violently rais'd in the ſaid perpendicular leg.

But if you gently raiſe the Syphon again, the lateral preſſure of the water againſt the immers'd Orifice being diminiſh'd, (according as the diſtance of that Orifice *G* from the *Horizontal* Surface, *AB*, comes to be leſſen'd,) the prevalent oyle will drive out the water, firſt out of the Longer leg, and then out of the ſhorter, and will at length flow out in drops at the immers'd Orifice, and thence emerge to the Top of the water.

Beſides, when the oyle in the Syphon does juſt counterballance the external water, if you keep the ſhorter leg parallel to the Surface of the water, and move the Orifice of it this way or that way, and place it nearer or further off from the middle or from the ſides of the Glaſs, (provided you keep it always at the ſame depth under the water,) you'l find the oyl in the longer Leg to continue (as to ſenſe) at the ſame height: Whence we may learn (what I have not yet found mention'd by any Writer,) That, ev'n in the mid'ſt of the water, we may ſuppoſe a pillar of *water*, of a Baſis equal to the ſide of an immers'd Body, (and reaching to the loweſt part of it;) And that, though this Imaginary aqueous pillar, ſuch as in our figure *G H*, be not included in any ſolid Body or ſtable ſuperficies; nevertheleſs its lower parts will have a lateral preſſure tending outwards, againſt the imaginary ſides, from the weight of the water that is above theſe ſubjacent and lateral parts; and will have that preſſure increas'd proportionably to the height to which the imaginary pillar reaches above them. Which obſervation, being duely noted and apply'd, may be of no mean uſe in the explication of divers Hydroſtatical phænomena.

And laſtly if, in ſtead of holding *E F*, the longer leg of our Syphon, perpendicular, (and, conſequently, the ſhorter parallel to the Horizon,) you varioſlly incline the former, ſo as to bring it to make an obtuſe or an Acute Angle with the ſuperficies of the water *A B*; though by this means the ſhorter and immers'd leg, *F G*, will in Situation ſometimes reſpect the Bottom, and ſometimes the top of the Glaſs: yet in all theſe oblique ſituations of this leg, and the immers'd Orifice of it, *G*, the oblique preſſure of the water will ſo much depend upon the height of the Surface of the Liquor above the Orifice, and ſo much conform to the obſervations already deliver'd, That you ſhall ſtill ſee the ſurface of the oyle *I K*, in the longer pipe, to be a little, and but a little ſuperiour to that of the external water, *A B*, and ſo the *Æquilibrium* betwixt the Liquor, or Liquors, within the Syphon, and the water without it, will ev'n in this caſe alſo be maintain'd.

SCHOLIUM.

REmembring on this Occaſion an Experiment, which though it do not ſhew what the precíſe quantity of Lateral preſſure is, that the lower parts of the fluid may ſuſtain from the more elevated ; yet it may confirm the foregoing Paradox , and by its Phænomena afford ſome hints that may render it not unacceptable ; I ſhall ſubjoyn it, as I ſet it down not long after I devis'd it.

In the firſt place then, there was made a glaſs Bubble with a ſlender neck ; and (in a word) of the figure expreſs'd in the annex'd Scheme ; This Bubble I caus'd to be ſo poys'd, That, though it would float upon the water, yet the addition of a weight ſmall enough would ſuffice to make it ſinck.

Fig. 14.

This done, I provided a very large wide mouth'd Glaſs, and caus'd to be fitted to it, as exactly as I could, a ſtopple of Cork, which being ſtrongly thruſt in, would not eaſily be lifted up. In the middle of this Cork there was burn'd, with a heated inſtrument, a round hole; through which was thruſt a long ſlender pipe of Glaſs; ſo that the lower end of it was a pretty way beneath the Cork, and the upper part of it was, as near as could be, at right Angles with the upper part of the ſaid Cork. And in an other part of the ſtopple, near the edge, there was made another round hole, into which was likewiſe thruſt another ſmall pipe; whoſe lower part reach'd alſo a pretty way beneath the Cork, but its upper part was but about two or three Inches high; and the Orifice of this upper part was carefully clos'd with a ſtopple and Cement. Then the glaſs veſſel being fill'd with water, and the pois'd Bubble being made to float upon it, the ſtopple or cover of the great glaſs veſſel was put on, and made faſt with a cloſe Cement, that nothing might get in or out of the veſſel, but at the long ſlender pipe; which was faſtned into the Cork (as was alſo the ſhorter pipe) not only by its own fitneſs to the hole it paſs'd through, but by a ſufficient Quantity of the ſame Cement, carefully apply'd to ſtop all creveſſes.

The Inſtrument thus prepar'd,(and inclin'd this or that way, till the floating Bubble was at a good diſtance from that end of the long pipe, which reached a pretty way downwards beneath the Surface of the water,) we began to pour in ſome of that Liquor at the open Orifice of the pipe *E F*; and, the mouth of the Veſſel being exactly ſtopp'd, the water for want of another place to receive it, aſcended into the pipe through which it had fallen before. And, if I held my hand when the water I had pour'd in was able to reach but to a ſmall height in the Cylinder, as for inſtance, to the Superficies *J*; the Bubble *X* would yet continue floating. But if I continued pouring till the water in the pipe had attain'd to a conſiderable height above the Surface of that in the Veſſel, as if it reach'd to K; then the Bubble *X* would preſently ſinck to the bottome of the Veſſel; and there continue, as long as as the water continued at ſo great a height in the pipe *E F*.

This Experiment will not only teach us, That the upper parts of the water gravitate upon thoſe that are under them, but (which is the thing we are now to confirm) That in a Veſſel, that is full, all the lower parts are preſs'd by the upper, though theſe lower be not directly beneath the upper, but aſide of them, and perhaps at a good diſtance from the Line in which they directly preſs: Theſe things, I ſay, may be made out by our Experiment. For the Addition of the Cylinder of water *K J*, in the pipe *E F*, makes the Bubble *X* ſubſide; as the force or preſſure of any other heavy body upon the water in the veſſel would do. And ſince (as may be gather'd from the Reaſon formerly given (in the Proof of the ſecond Paradox) of the ſincking of pois'd Bubbles) the included aire in our Bubble was notably compreſs'd; it will follow, that the Cylinder of water, *KI*, did preſs the ſubjacent water in the Veſſel. For, without ſo doing, it could not be able to compreſs the aire in the Bubble. And ſince the ſaid Bubble did not ſwimme directly under or near the pipe *E F*; but at one ſide of it, and at a pretty diſtance from it, nay and floated above the lower Orifice, *F*, of the pipe; 'tis evident that that Aqueous Cylinder, *J*K, does not only preſs upon the water, or other Bodies that are directly under it; but upon thoſe alſo that are laterally ſituated in reſpect of it, provided they be inferior to it.

And, according to this Doctrine, we may conceive, that every aſſignable part of the ſides of the Veſſel does ſuſtaine a preſſure, encreas'd by the encreaſe of that parts depth under water, and according to the largneſs of the ſaid part. And therefore, if any part were ſo weak, as that it would be eaſily beaten out or broken by a weight equal to the Cylinder *I* K, (making always a due abatement for the obliquity of the preſſure) it would not be fit to be a part of our Veſſel: Nay the Cork it ſelf, though it be above the Surface of the water in the Veſſel; yet becauſe the water in the pipe is higher then it, each of its parts reſiſts a conſiderable preſſure proportionate to its particular bigneſs, and to the height of the water in the pipe. And therefore, if the Cork be not well ſtopp'd in, it may be lifted up by the preſſure of the water in the pipe, if that be fill'd to a good height. And if the Cement be not good and cloſe, the water will (not without noiſe) make it ſelf a paſſage through it. And if the ſtopple *G*, of the ſhorter pipe *G H*,(which is plac'd there likewiſe to illuſtrate the preſent conjecture) do not firmly cloſe the Orifice of it, it may be forced out, not without violence and noiſe. And, for further ſatisfaction, if, in ſtead of the ſtopple *G*, you cloſe the Orifice with your finger, you ſhall find it preſs'd upwards as ſtrongly, as it would be preſs'd downwards by the weight of a Cylinder of water of the breadth of the pipe, and of a not inconſiderable height, (for 'tis not eaſie to determine preciſely, what height :) ſo that (to be ſhort) in the fluid Body, we made our tryal with, the preſſure of the Superior parts was communicated, not onely to thoſe that were plac'd *directly* under them, but ev'n to thoſe that were but *obliquely* ſo, and at a diſtance from them.

I had forgot to confirm, that it was the preſſure of the ſuperiour parts of the water, that made our floating bubble ſinck, by ſuch another circumſtance as I took notice of in ſome of the former Experiments; *viz.* that, when it lay quietly at the bottom of the Veſſel, if by inclining the Inſtrument we pour'd off as much of the water in the pipe, *E F*, as ſuffic'd competently to diminiſh its height above the water in the Veſſel *A B C D*, the air in the bubble finding its former preſſure alleviated, would preſently expand it ſelf, and make the bubble emerge. And to ſhow, That the very oblique preſſure w^{ch} the bubble ſuſtain'd from the water in the pipe, was not overmuch differing from that which it would have ſuſtain'd from an External force, or from the weight of water plac'd directly over it; I caus'd two ſuch bubbles to be pois'd, and having put each of them into a long Cylindrical Glaſs, open above, and fill'd with water, upon which it floated, if we thruſt it down a little way it would (agreeably to what hath been above related) aſcend again; ſo that we were forc'd to thruſt it down to a good depth, before the preſſure of the incumbent water was great enough to make it ſubſide.

See the Proof of the II. Paradox.

And perhaps it will not be impertinent to take notice, before we conclude, how the preſſure of ſuch differing fluids, as aire and water, may be communicated to one another. For having ſometimes forborn to fill the Veſſel *A B C D* quite full of water, ſo that, when the Cork was fitted to it, there remain'd in it a pretty quantity of aire,(as between the Surface *L M*, and the Cork) nevertheleſs, if the ſtopple or cork were very cloſely put in, the preſſure of the water that was afterwards poured into the pipe *E F*, from *J* to K, would make the bubble ſinck, little otherwiſe, for ought I took notice of, then if the Veſſel had been perfectly fill'd with water; the aire (above *L M*,) that was both impriſoned and compreſs'd, communicating the preſſure it receiv'd to the water contiguous to it.

PARADOX VIII.

That water may be made as well to depreſs a Body lighter then it ſelf, as to buoy it up.

HOw ſtrange ſoever this may ſeem, to thoſe that are prepoſſeſs'd with the vulgar Notions about gravity and levity: It need not be marvail'd at, by them that have conſider'd what has been already deliver'd. For ſince, in Fluid Bodies, the upper parts preſs upon the lower, and upon other bodies that lie beneath them. And ſince, when a Body is unequally preſs'd by others, whether lighter or heavier then it ſelf, it muſt neceſſarily be thruſt out of that place, where it is more preſs'd, to that where 'tis leſs preſs'd; If that a parcel of oyle be by a contrivance ſo expoſed to the water, as that the water preſſes againſt its upper Superficies, and not againſt the undermoſt or lateral parts of it; If we ſuppoſe that there is nothing (whoſe preſſure is not inferiour to that of the water) to hinder its deſcent, (ſuppoſing, withal, that the oyle and water cannot paſs by one another; for which cauſe, we make uſe of a ſlender pipe;) the oyle muſt neceſſarily give way downwards, and conſequently be depreſs'd and not buoy'd up. This is eaſily exemplified by the following Experiment.

Take a ſlender Glaſs Syphon *E F G H*, of the bore we have often mention'd, whoſe ſhorter leg *G H* may be about 3 or 4 Inches long, and as parallel as the Artificer can make it to the longer *E F*; dip the ſhorter leg in oyle of Turpentine, till the oyle quite

Fig. 15.

quite fill the ſhorter leg, and reach to an equal height in the longer, as from *F* to *J*. Then ſtopping the Orifice *E* of the longer leg with your finger, and immerſing the repleniſh'd part of the Syphon about an inch under water, you ſhall perceive that as you thruſt it lower and lower, upon the removal of your finger, the oyle in the ſhorter leg will be made to ſinck about an inch or ſomewhat more; and as afterwards you thruſt the pipe deeper, the oyle in the ſhorter leg will, by the weight of the incumbent water, *H K*, be driven downward more and more, till it come to the very bottom of the ſhorter leg; whence, by continuing the immerſion, you may impel it into the longer. The cauſe of which Phænomenon, I ſuppoſe to be already clearly enough aſſign'd, to make it needleſs to add any thing here about it.

It remains, that, before I proceed

to the next propoſition, I add; That, to Exemplifie at once three Paradoxes, (both this, and the next foregoing, and the ſecond) I cauſ'd to be made a ſlender Glaſs-pipe, of the Figure expreſs'd in the annexed Scheme, and having, by the lower Orifice *L*, ſuck'd into it as much oyle of Turpentine, as reach'd in the longeſt leg, *N O*, as high as the Top of the other part of the Glaſs; (namely, to the part *P*, in the ſame level with the Orifice *L*,) I firſt ſtopp'd the upper Orifice of it, *O*, with my finger. And then, thruſting it as before under water to a convenient depth, upon the removal of my finger, the External water did firſt drive away the oyle that was in *L M*, that part of the crooked pipe which was parallel to the Horizon; then it depreſs'd the ſame oyle to the bottom of the ſhorter leg, that is from *M* to *N*: And laſtly,

Fig. 16.

it impell'd it all up into the longer leg *N P O*, to what height I thought fit. So that the oyle was preſs'd by the water both laterally, downwards, and upwards: the cauſes of which are eaſily deducible from the Doctrine already deliver'd.

PARA-

PARADOX IX.

That, what ever is ſaid of poſitive Levity, a parcel of oyle lighter then water, may be kept in water without aſcending in it.

TO make out what I have to repreſent about this Paradox the more intelligible, the beſt way perhaps will be to ſet down the Conſiderations that induc'd me to judge the thing it pretends to feaſible. And in order to this, it would be expedient to conſider, why it is that a Body lighter *in ſpecie* then water, being plac'd never ſo much beneath the Superficies of that Liquor, will rather emerge to the

Top, then ſinck to the bottom of it; if we had not already conſider'd that problem in the Explication of the third Paradox. But being now allow'd to apply to our preſent purpoſe what hath been there deliver'd, I ſhall forthwith ſubjoyne, That 'twas eaſie enough for me to collect from hence, that,the Reaſon why it ſeems not poſſible, That a parcel of oyle lighter then water, ſhould without violence be kept from emerging to the Top of it, being this, *That ſince the Surface of a Veſſel full of ſtanding water is (Phyſically ſpeaking) Horizontal, the water that preſſes againſt the lower part of the immers'd Body, muſt needs be deeper then that which preſſes againſt the upper*: If I could ſo order the matter,that the water that leans upon the upper part of the Body ſhould, by being higher then the level of the reſt of the water, have a height great enough to ballance that which preſſes againſt the lower, (and the Bodies not ſhift places,by paſſing one by the other) the oyle might be kept ſuſpended betwixt two parcels of water.

To reduce this to practiſe, I took the following courſe; having ſuck'd into a ſlender pipe (ſuch as that imploy'd about the firſt experiment) about an Inch of water, and kept it ſuſpended there by ſtopping the Orifice of the pipe; I thruſt the lower part of the pipe about two inches beneath the Surface of ſome oyl of Turpentine (which, to make the effect the clearer, I ſometimes tinge deeply with Copper:) then removing my finger, the oyle being preſs'd againſt the immers'd Orifice with a greater force, then the weight of ſo little ſuſpended water could reſiſt, that oyle was impell'd into the lower part of the pipe to the height of near an inch; and then again I ſtopp'd the upper Orifice of the pipe with my

finger,

finger; and thereby keeping both the Liquors ſuſpended in it, I thruſt the pipe into a Glaſs full of water, three or four inches beneath the Surface of it; and then (for the Reaſon juſt now given) the water, upon the Removal of my finger, will preſs in at the lower Orifice of the pipe, and impell up the *Fig.* 17. oyle, till they come to ſuch a ſtation, as that expreſs'd in the annex'd Scheme: where *P Q* is the water, newly impell'd up into the pipe, *Q R* is the oyl, and *R S* the water that was at firſt ſuck'd into the pipe. For in this ſtation, theſe three liquors do altogether as much gravitate upon the part *P*, as the incumbent water alone does upon the other parts of the imaginary ſuperficies *G H*; and yet the oyle, *R Q*, does not aſcend, becauſe the diffluence of the water, *R S*, being hindred by the ſides of the pipe, its ſuperficies, *T S*, is higher then *A D*, the

Superficies of the reſt of the water; by which means the incumbent water may be brought to have upon the upper part *R* of the oleous Cylinder, as great a preſſure as that of the water, that endeavours to impel upwards the lower part *Q* of the ſame ſuſpended Cylinder of oyle.

PARA-

PARADOX X.

That the cauſe of the Aſcenſion of water in Syphons, and of its flowing through them, may be explicated without having a recourſe to natures abhorrency of a Vacuum.

BOth Philoſophers and Mathematicians, having too generally confeſt themſelves reduc'd to fly to a *fuga vacui*, for an account of the cauſe of the running of water and other Liquors through Syphons. And ev'n thoſe moderns, that admit a *Vacuum*, having (as far as I have met with) either left the Phænomenon unexplicated, or endeavour'd to explain it by diſputable Notions: I think the Curious much oblig'd to

Monſieur *Paſchal*, for having ingeniouſly endeavour'd to ſhew, That this difficult Probleme need not reduce us to have recourſe to a *fuga vacui*. And indeed his Explication of the motion of water in Syphons, ſeems to me ſo conſonant to Hydroſtatical principles, that I think it not neceſſary to alter any thing in it. But as for the experiment he propounds to juſtifie his Ratiocination, I fear his Readers will ſcarce be much invited to attempt it. For, beſides that it requires a great quantity of Quickſilver; and a new kind of Syphon, 15 or 20 foot long; the Veſſels of Quickſilver muſt be plac'd 6 or 7 yards under water, that is, at ſo great a depth, that I doubt whether men, that are not divers, will be able conveniently to obſerve the progreſs of the Tryal.

Wherefore we will ſubſtitute a way, which may be try'd in a glaſs Tube, not two foot deep, by the help of another peculiarly contriv'd glaſs, to be prepar'd by a skilful hand. Provide then a glaſs Tube *A B C D*, of a good wideneſs, and half a yard or more in depth; provide alſo a Syphon of two legs *F* K, and K *G*, whereunto is joyn'd (at the upper part of the Syphon) a pipe *E* K, in ſuch manner, as that the Cavity of the pipe communicates with the cavities of the ſyphon; ſo *Fig.* 18. that if you ſhould pour in water at *E*, it would run out at *F* and *G*. To each of the two Legs of this new Syphon, muſt be ty'd with a ſtring a pipe of Glaſs, *I* and H, ſeal'd at one end, and open at the other; at which it admits a good part of the leg of the Syphon to which it is faſtned, and which leg muſt reach a pretty way beneath the Surface of the water, wherewith the ſaid pipe is to be almoſt fill'd. But as one of theſe legs is longer then the other, ſo the ſurface of the water in the ſuſpended pipe *I*, that is faſtned to the ſhorter leg *KF*, muſt be higher (that is, nearer to K or *A B*) then the ſurface of the water in the pipe H, ſuſpended from the longer leg KG; that (according to what is uſual in Syphons) the water may run from a higher veſſel to a lower.

All things being thus provided; and the pipe *E* K being held, or otherwiſe made faſt that it may not be mov'd; you muſt gently poure oyle of Turpentine into the Tube *A B C D*, (which, if you have not much oyle, you may before hand fill with water till the liquor reach near the Bottom of the ſuſpended pipes, as to the ſuperficies *X Y*) till it reach higher then the top of the Syphon *F K G*, (whoſe Orifice *E* you may, if you pleaſe, in the mean time cloſe with your finger or otherwiſe, and afterwards unſtop) and then the oyle preſſing upon the water will make it aſcend into the legs of the Syphon ; and paſs through it, out of the uppermoſt veſſel *J*, into the lowermoſt H ; and if the veſſel *J* were ſupply'd with water, the courſe of the water through the Syphon would continue longer, then here (by reaſon of the paucity of water) it can do.

Now in this Experiment we manifeſtly ſee the water made to take its courſe through the legs of a Syphon from a higher veſſel into a lower , and yet the top of the Syphon being perforated at K, the aire has free acceſs to each of the legs of it, through the hollow pipe *E* K which communicates with them both. So that, in our caſe, (where there is no danger of a *Vacuum*, though the water ſhould not run *through* the Syphon) the fear of a *Vacuum* cannot with any ſhew of Reaſon be pretended to be the cauſe of its running. Wherefore we muſt ſeek out ſome other.

And it will not be very difficult to find, that 'tis partly the preſſure of the oyle, and partly the contrivance and ſituation of the veſſels; if we will but conſider the matter ſomewhat more atentively. For the oyle, that reaches much higher then *K*, and conſequently then the leggs of the Syphon, preſſes upon the ſurface of the External water, in each of the ſuſpended pipes *I* and *H*. I ſay the *External water*, becauſe the oyle floating upon the water, and the Orifices of both the legs *F* and *G* being immers'd under the water, the oyle has no acceſs to the cavity of either of thoſe legs. Wherefore, ſince the oyle gravitates upon the water without the legs, and not upon that within them, and ſince its height above the water is great enough to preſs up the water into the Cavity of the legs of the Syphon, and impel it as high as *K*, the water muſt by that preſſure be made to aſcend.

And this raiſing of the water happening at firſt in both legs, (for the cauſe is in both the ſame) there will be a kind of conflict about K betwixt the two aſcending portions of water, and therefore we will now examine which muſt prevaile.

And if we conſider, That the preſſure, ſuſtein'd by the two parcels of water in the ſuſpended pipes *I* and *H*, depends upon the height of the oyle that preſſes upon them reſpectively; it may ſeem (at the firſt view) That the water ſhould be driven out of the lower veſſel into the higher. For if we ſuppoſe that part of the ſhorter leg that is unimmers'd under water to be 6 Inches long, & the unimmers'd part of the longer leg to be ſeaven Inches; becauſe the ſurface of the water in the veſſel *I*, is an Inch higher, then that of the water in the veſſel *H*, it will follow, That there is a greater preſſure upon the water, whereinto the longer leg is dip'd, by the weight of an Inch of oyle: ſo that that liquor being an inch higher upon the ſurface of the water in the pipe *H*, then upon that in the pipe *I*, it ſeems that the water ought rather to be impell'd from *H* towards *K*, then from *I* towards *K*.

But then we muſt conſider, That, though the deſcent of the water in the leg *G*, be more reſiſted then that in the other leg, by as much preſſure as the weight of *an Inch of oyle* can amount to; Yet being longer by an Inch then the water in the leg *F*, it tends downwards more ſtrongly by the weight of an *Inch of water*, by which length it exceeds the water in the oppoſite leg. So that an inch of water being *(cæteris paribus)* heavier then an Inch of oyle; the water in the longer leg, notwithſtanding the greater reſiſtance of the external oyle, has a ſtronger endeavour downwards, then has the water in the ſhorter leg; though the deſcent of this be reſiſted but by a depth of oyl leſs by an Inch. So that all things computed, the motion muſt be made towards that way where the endeavour is moſt forcible; and conſequently the courſe of the water muſt be from the upper veſſel, and the ſhorter leg, into the longer leg, and ſo into the lower veſſel.

The application of this to what happens in Syphons is obvious enough. For, when once the water is brought to run through a Syphon, the aire (which is a fluid, and has ſome gravity, and has no acceſs into the cavity of the Syphon,) muſt neceſſarily gravitate upon the water whereinto the legs of the Syphon are dip'd, and not upon that which is within the Syphon: and conſequently, though the incumbent aire have ſomewhat a greater height upon the water in the lower veſſel, then upon that in the upper ; yet the gravitation it thereby exerciſes upon the former more then upon the latter, being very inconſiderable, the water in the longer leg much preponderating (by reaſon of its length) the water in the ſhorter leg, the efflux muſt be out of that leg, and not out of the other. And the preſſure of the External aire being able to raiſe water (as we find by ſucking Pumps) to a far greater height, then that of the ſhorter leg of the Syphon ; the efflux will continue, for the ſame reaſon, till the exhauſtion of the water, or ſome other circumſtance, alter the caſe. But, if the legs of the Syphon ſhould exceed 34 or 35 foot of perpendicular altitude ; the water would not flow through it ; the preſſure of the external aire being unable, (as

In the Phyſico-Mechanical Experiments.

has

has been elſewhere declared,) to raiſe water to ſuch a height. And if a hole being made at the top of a Syphon, that hole ſhould be unſtopp'd while the water is running, the courſe of it would preſently ceaſe. For, in that caſe, the aire would gravitate upon the water, as well within as without the cavity of the Syphon; and ſo the water in each leg would, by its own weight, fall back into the veſſel belonging to it.

But becauſe this laſt circumſtance, though clearly deducible from Hydroſtatical principles and Experiments, has not, that I know of, been verified by particular Tryals, I caus'd two Syphons to be made, the one of Tin, the other of Glaſs; each of which had, at the upper part of the flexure, a ſmall round hole or ſocket, which I could ſtop and unſtop, at pleaſure, with the pulp of my finger. So that, when the water was running through the Syphon, in caſe I remov'd my finger, the water would preſently fall, partly into one of the ſubjacent veſſels, and partly into the other. And if the legs of the Syphon were ſo unequal in length, that the water in the one had a far greater height (or depth) then in the other; there ſeem'd to be, when the liquor began to take its courſe through the Syphon, ſome light preſſure from the external aire upon the finger, wherewith I ſtopp'd the Orifice of the ſocket made at the flexure.

And on this occaſion I will add, what I more then once try'd; to ſhew, at how very minute a paſſage the preſſure of the External aire may be communicated, to Bodyes fitted to receive it. For, having for this purpoſe ſtopp'd the orifice of one of the above mentioned Syphons, (inſteed of doing it with my finger,) with a piece of oyl'd paper, carefully faſtned with Cement to the ſides of the ſocket; I found, as I expected, that though hereby the Syphon was ſo well clos'd, that the water ran freely through: yet, if I made a hole with the point of a needle, the aire would at ſo very little an orifice inſinuate it ſelf into the cavity of the Syphon, and, thereby gravitating as well within as without, make the water in the legs to fall down into the veſſels. And *though*, if I held the point of the needle in the hole I made, and then caus'd one to ſuck at the longer leg; this ſmall ſtopple, without any other help from my hand, ſuffic'd to make the Syphon fit for uſe: *Yet* if I remov'd the needle, the aire would (not without ſome noiſe) preſently get in at the hole, and put a final ſtop to the courſe of the water. Nor was I able to take out the needle and put it in again ſo nimbly, but that the aire found time to get into the Syphon; and, till the hole were again ſtopp'd, render it uſeleſs, notwithſtanding that the water was by ſuction endeavour'd to be ſet a running.

PARA-

PARADOX XI.

That a ſolid Body, as ponderous as any yet known, though near the Top of the water it will ſinck by its own weight; yet if it be plac'd at a greater depth then that of twenty times its own thickneſs, it will not ſinck, if its deſcent be not aſſiſted by the weight of the incumbent water.

THis Paradox, having never been (that I know of) propos'd as yet by any, has ſeem'd ſo little credible to thoſe to whom I have mention'd it, (without excepting Mathematicians themſelves,) that I can ſcarce hope it ſhould be readily and generally received in this Illuſtrious Company, upon leſs clear Teſtimony, then that of Experience. And therefore, though (if I miſtake not) ſome part of this propoſition may be plauſibly deduc'd by the help of an Inſtrument ingeniouſly thought upon by *Monſieur Paſchal* ; Yet I ſhall have recourſe to my own Method for the making of it out, for theſe two Reaſons. The one, That a great part of the Paradox muſt be Explicated, as well as prov'd, by the Doctrine already ſetled in this paper. The other, That the Experiment propos'd by *Monſieur Paſchal*, being to be done in a deep River, and requiring a Tube 20 foot long, whoſe Bottome muſt be fitted with a Braſs Cylinder, made with an exactneſs, ſcarce (if at all) to be hoped for from our Workmen: If I ſhould build any thing on this ſo difficult an Experiment, (which himſelf does not affirm to have ever been actually tryed,) I fear moſt men would rather reject the Experiment as a Chimærical thing, then receive for its ſake a Doctrine that appears to them very Extravagant.

Let us then, to imploy in this caſe alſo the method we have hitherto made uſe of, Fill a Glaſs veſſel, *A B C D*, almoſt full of water; only, in regard that there is a great depth of water requiſite to ſome Circumſtances of the Experiment, This laſt muſt not be ſo ſhallow as thoſe hitherto imploy'd: but a deep Cylinder, or Tube ſeal'd at one end, whoſe depth muſt be at leaſt two or three foot, though its breadth need not be above 2 or 3 Inches; and, to keep it upright, it may be plac'd in a ſocket of metal or wood, of a ſize and weight convenient for ſuch a purpoſe. This Glaſs being thus fitted in water, let us ſuppoſe *E F*, to be a round and flat piece of ſolid Braſs, having about an

Fig. 19.

Inch in Diameter, and a fourth or ſixth part of an inch in thickneſs. This Cylinder, being immers'd under water till it be juſt cover'd by the uppermoſt Surface of that Liquor, and being let go, muſt neceſſarily fall downwards in it; becauſe if we ſuppoſe the imaginary Superficies, *G H*, to paſs along the Circle *F*, which is the lower part of the Braſs Body, that metal being *in ſpecie* far heavier then water, the Braſs that leans upon the part *F*, muſt far more gravitate upon the ſaid part *F*, then the incumbent water does upon any other part of the Superficies *G H*; and, conſequently, the ſubjacent water at *F* will be thruſt out of place by the deſcending Body. And becauſe that, in what part ſoever of the water, not exceeding nine times its thickneſs meaſured from the Top of the water *A C*, the ponderous Body, *E F*, ſhall happen to be; there will be ſtill, by reaſon of the ſpecifick gravity of the Metal, a greater preſſure upon that part of the imaginary Superficies that paſſes along the bottome of the Body on which the part *F* ſhall happen to lean, then upon any other part of the ſame imaginary Superficies; the Braſs Body would ſtill deſcend by vertue of its own weight, though it were not aſſiſted by the weight of the water that is over it. But let us ſuppoſe it to be plac'd under water on the deſignable plain *J* K; and let this plain, which (as all other imaginary plains) is, as well as the real Surface of the water, to be conceiv'd parallel to the Horizon; and let the depth or diſtance of this plaine, from the uppermoſt Surface of the water, be (ſome what) above nine times the thickneſs of the Braſs Body: I ſay that, in this caſe, the body would not deſcend, if it were not preſs'd downwards by the weight of the water it has over it. For Braſs being but about nine times * as heavy as water of an equal bulk to it, the Body *E F* alone would preſs upon the part *F*, but as much as a Cylinder of water would, which having an equal Baſis were 8 or 9 times as high as the Braſs is thick. But now all the other parts of the Imaginary ſurfaces, *I K*, being preſs'd upon by the incumbent water, which is as high above them as the newly mention'd Cylinder of water would be; there is no reaſon why the part *F* ſhould be depreſs'd, rather then any other part of the Superficies *J* K: But becauſe it is true, which we formerly taught; namely, that water retains its gravity in water; and that too, though a body, heavier *in ſpecie* then it, be plac'd

imme-

** The word, about, is added, becauſe indeed the Author, as he elſewhere delivers, did by exact ſcales find Braſs to weigh between eight or nine times as much as water; but judg'd it needleſs to his preſent Argument, and inconvenient to take notice of the fraction.*

immediately under it ; it will neceſſarily happen, That in what part ſoever the ſolid body be plac'd, provided it be every way environ'd with the water, it muſt, for the Reaſon newly given, be made to move downwards, partly by its own weight, and partly by that of the incumbent water ; and muſt continue to ſinck, till it come to the bottom, or ſome other body that hinders its farther deſcent.

But in caſe the water above the ſolid body did not gravitate upon it, and thereby aſſiſt its deſcent ; or, in caſe that the incumbent water were by ſome Artifice or other, ſo remov'd, That none of the lateral water (if I may ſo call it) could ſucceed in its place to lean upon the ſolid ; then it will follow, from what we have newly ſhown, that the ſolid would be kept ſuſpended. And in caſe it were plac'd much deeper in the water, as over againſt the point *L* or *M*; Then, if we conceive the incumbent water to be remov'd or fenc'd off from it, the preſſure of the ſolid alone upon the part *F*, of the imaginary Superficies *L M*, being very much inferior to that of the water upon the other parts of the ſame Surface, the part *F* would be ſtrongly impell'd upwards, by a force proportionate to the difference of thoſe two preſſures. And therefore, ſince I have found by tryals, purpoſely made in ſcales marvellouſly exact, and with refined Gold, (purer then perhaps any that was ever weighed in water) That Gold, though much the ponderouſeſt of bodies yet known in the world, is not full 20 times as heavy as water of the ſame Bulk; I kept within compaſs (as well as imploy'd a round number, as they call it) when I ſaid, That no body (yet known,) how ponderous ſoever, will ſubſide in water by its own weight alone, if it were ſo plac'd under water, that the depth of the water did above twenty times exceed the height of the Body; (not to mention here, that though gold and water being weigh'd in the aire, their proportion is above 19 to one, yet in the water, gold does, as other ſincking bodies, looſe as much of its weight, as that of an equal bulk of water amounts too.)

I was ſaying juſt now, that in caſe the Brazen body were plac'd low enough beneath the Surface of the water, and kept from being depreſs'd by any incumbent water, it would be ſupported by the ſubjacent water. And this is that very thing that I am now to ſhew by an Experiment.

Let then the Braſs body *E F*, be the cover of a braſs Valve; (as in the annexed figure:) and let the Valve be faſtned with ſome ſtrong and cloſe Cement

Fig. 20.

to a Glaſs pipe, *O P*, (open at both ends) and of a competent length and wideneſs. For then the Body, *E F*, being the undermoſt part of the Inſtrument, and not ſticking to any other part of it, will fall by its own weight if it be not ſupported. Now then, tying a thred to a Button *Q*, (that is wont to be made in the middle of the doors of Braſs valves) you muſt, by pulling that ſtring ſtreight and upwards, make the Body, *E F*, ſhut the orifice of the Valve, as cloſe as you can; (which is eaſily and preſently done.) Then thruſting the Valve under water, to the depth of a foot or more; the Cement and the ſides of the Glaſs, *O P*, (which reaches far above the top of the water *X Y*) will keep the water from coming to beare upon the upper part of the body *E F*; and conſequently the imaginary Surface, *V W*, (that paſſes by the lower part of the ſaid body) will, where it is contiguous thereunto, be preſs'd upon only by the proper weight of the body *E F*; but in its other parts, by the much greater weight of the incumbent water. So that, though you let go the ſtring, (that held the body, *E F*, cloſe to the reſt of the Inſtrument) the ſaid body will not at all ſinck, though there be nothing but water beneath it to ſupport it.

And to manifeſt that 'tis onely the preſſure of the water, of a competent depth, that keeps the ſolid ſuſpended; if you ſlowly lift up the inſtrument towards (X *Y*) the top of the water; you ſhall find, that, though for a while the parts of the Valve will continue united, as they were before; yet, when once it is rais'd ſo near the Surface, (as between the plain *J* K, and *X Y*) that the ſingle weight of *E F*, upon the ſubjacent part of the imaginary plain that paſſes by it, is greater then the preſſure of the incumbent water upon other parts of the ſame plain; that Body, being no more ſupported as formerly, will fall down, and the water will get into the pipe, and aſcend therein, to the level of the External water.

But if, when the Valve is firſt thruſt under water, and before you let go the thred that keeps its parts together; you thruſt it down to a good depth, as to the Superficies *R S*: then, though you ſhould hang a conſiderable weight, as *L*, to the Valve *E F*, (as I am going to ſhew you a Tryal with a Maſſy Cylinder of ſtone broader then the Valve, and of divers inches in length) the ſurpluſage of preſſure on the other parts of the plain, *V W*, (now in *R S*) over and above what the weight of the body *E F*, and that of the Cylindrical ſtone, *L*, to boot, can amount to, on that part of the Surface vvhich is contiguous to the ſaid body *E F*, will be great enough to preſs ſo hard againſt the lower part of the Valve, that its own weight, though aſſiſted with that of the ſtone, will not be able to diſjoyne them.

By which (to note that by the way) you may ſee, that though, when two flat and poliſh'd marbles are joyn'd together, we find it is impoſſible to ſever them without force; we need not have recourſe to a *fuga vacui*, to Explicate the cauſe of their Cohæſion, whilſt they are environ'd by the Aire, which is a Fluid not devoid of Gravity, and reaching above the Marbles no body knows how high.

And to evince, That 'tis only ſuch a preſſure of the water, as I have been declaring, that cauſes the Cohæſion of the parts of the Valve; if you gently lift it up towards the top of the water, you will quickly find the Braſs body, *E F*, drawn down by the ſtone (*L*) that hangs at it; as you will perceive by the waters getting in between the parts of the Valve, and aſcending into the pipe.

To which I ſhall only add, what you will quickly ſee, That, in perfect Conformity to our Doctrine, the preſſure of the body, *E F*, upon the ſubjacent water, being very much increaſed by the weight of the ſtone that hangs at it, the Valve needs not, as before, be lifted up above the plain *J K*, to overcome the reſiſtance of the water, being now enabled to do it before it is rais'd near ſo high.

APPENDIX I.

Containing an Anſwer to ſeven Objections, propos'd by a late Learned Writer, to evince, that the upper parts of water preſs not upon the lower.

AFter I had, this Morning, made an end of reviewing the foregoing papers, there came into my hands ſome queſtions lately publiſh'd, among other things, by a very recent Writer of Hydroſtaticks. In one of which Queſtions, the Learned Author ſtrongly defends the contrary to what has there been in ſome places prov'd, and divers places ſuppos'd.

The Author of theſe Erotemata

aſſerts, That, *in conſiſtent water, the upper parts do not gravitate or preſs upon the lower.*

And therefore, I think it will be neither uſeleſs, nor improper, briefly to examine here the Arguments he produces. Not uſeleſs; becauſe the Opinion he aſſerts, both is, and has long been, very generally receiv'd; and becauſe too, it is of ſo great importance, that many of the Erroneous Tenets and Concluſions, of thoſe that (whether profeſſedly or incidentally) treat of Hydroſtatical matters, are built upon it. And not *improper*; becauſe our Learned Author ſeems to have done his Reader the favour to ſumme up into one page all the Arguments for his Opinions that are diſperſedly to be found in his own or others mens Books. So that in anſwering theſe, we may hope to do much towards a ſatisfactory Deciſion of ſo important a Controverſie. And, after what we have already deliver'd, our Anſwers will be ſo ſeaſonable, that they will not need to be long: The things they are built on having been already made out, in the reſpective places whereto the Reader is referr'd.

Our Author then maintains, that, in Conſiſtent water, the Superiour do not actually preſs the Inferiour parts, by the ſeven following Arguments.

Object. 1. Sayes he, *Becauſe elſe the inferiour parts of the water would be more denſe then the Superior, ſince they would be compreſs'd and condens'd by the weight of them.*

Anſ. But if the Corpuſcles, whereof water conſiſts, be ſuppos'd to be perfectly ſolid & hard; the inferior Corpuſcles may be preſs'd upon by the weight of the ſuperior, without being compreſs'd or condens'd by them. As it would happen, if Diamond duſt were lay'd together in a tall heap: For though the upper parts, being heavy and ſolid Corpuſcles, cannot be deny'd to lean and preſs upon the lower; yet theſe, by reaſon of their Adamantine hardneſs, would not be thereby compreſs'd. And 'tis poſſible too, that the Corpuſcles of water, though not ſo perfectly hard, but that they may a little yield to an extream force, be ſolid enough not to admit from ſuch a weight, as that of the incumbent water, (at leaſt in ſuch ſmall heights as obſervations are wont to be made in,) any compreſſion, great enough to be ſenſible; As, beſides ſome Tryals I have formerly mention'd in another place, thoſe made in the preſence of this Illuſtrious Company ſeem ſufficiently to argue; *viz.* That water is not ſenſibly compreſſible by an ordinary force. And I find not, by thoſe that make the Objection, that they ever took pains to try, whether in deep places of the Sea, the lower parts are not more condens'd then the upper: nor do I ſee any abſurdity, that would follow from admitting them to be ſo.

Object. 2. Our Authors ſecond Argument is, *Becauſe Divers feel not, under water, the weight of the water that lyes upon them.*

Anſ. But for Anſwer to this Argument, I ſhall content my ſelf to make a reference to the enſuing Appendix, where this matter will be conſidered at large; and where, I hope, it will be made to appear, that the phænomenon may proceed, partly from the firm Texture of the Divers body, and partly from the nature of that preſſure which is exercis'd againſt bodyes immers'd in fluids; which, in that caſe, (as to ſenſe) preſſes every where equally, againſt all the parts of the body, expos'd to their Action.

Object. 3. The third Argument is, *That ev'n the ſlighteſt Herbs growing at the bottom of the water, and ſhooting up in it to a good height, are not oppreſs'd or lay'd by the incumbent water.*

Anſ. But the Anſwer to that is eaſie, out of the foregoing Doctrine. For the Plants, we ſpeak of, ſuſtain not the preſſure of the water above them by their own ſtrength; but by the help of the preſſure of water that is beneath: which being it ſelf preſs'd by the water that is(though not perpendicularly over it) ſuperior to it, preſſes them upwards ſo forcibly, that if they were not by their Roots, or otherwiſe faſtned to the ground, they, being *in ſpecie* lighter then water, would be buoy'd up to the top of the water, and made to float; as we often ſee that weeds do, which ſtorms, or other accidents have torn from their native ſoyle.

Object. 4. A fourth Objection is this, *That a heavy Body ty'd to a ſtring, and let down under water, is ſupported, and drawn out with as much eaſe, as it would be if it had no water incumbent on it; nay, with greater eaſe, becauſe heavy bodyes weigh leſs in water then out of it.*

Anſ. But an Account of this is eaſie to be rendred out of our Doctrine; For, though the water incumbent on the heavy body do really endeavour to make it ſinck lower, yet that endeavour is reindred ineffectual, to that purpoſe, by the equal preſſure of the water upon all the other parts of the Imaginary ſurface, that is contiguous to the bottom of the immers'd body. And that preſſure upon the other parts of that ſuppos'd plain, being equal not only to the preſſure of the pillar of water,

ter, but to that pillar, and to the weight of as much water as the immers'd body fills the place of; it muſt needs follow, That not only the hand that ſuſteins the body, ſhould not feel the weight of the incumbent water, but ſhould be able to lift up the Body more eaſily in the water, then in the aire. But though the preſſure of the water incumbent on the ſtone can not, for the reaſon aſſign'd, be felt in the caſe propos'd; yet if you remove that water, (as in the Experiment brought for the proof of the laſt Paradox,) it will quickly appear by the preſſure againſt the lower part of the heavy body, and its inability to deſcend by its own weight, when it is any thing deep under water; it will (I ſay) quickly appear, by what will follow upon the abſence of the Incumbent water, how great a preſſure it exercis'd upon the ſtone whilſt it lean'd on it.

Object. 5. The fifth Argument is propos'd in theſe words, *Becauſe a Bucket full of water, is lighter in the water, then out of it; nor does weigh more when full within the water, then when empty out of it; nay it weighs leſs, for the reaſon newly aſſign'd* (in the fourth Objection;) *therefore the water of the Bucket, becauſe it is within water, does not gravitate, nor conſequently preſs downwards, either the Bucket, or the water under the Bucket.* This is the grand and obvious Experiment, upon which the Schools, and the generality of Writers, have very confidently built this Axiom: *That the Elements do not gravitate in their proper place*; and particularly, that water weighs not (as they ſpeak) in its own Element.

Anſ. What they mean by proper or *natural place*, I ſhall not ſtand to examine, nor to enquire whether they can prove, that water or any other ſublunary body poſſeſſes any place, but upon this account, that the cauſe of gravity, or ſome other movent, enables it to expel other contiguous Bodies (that are leſs heavie or leſs moved,) out of the place they poſſeſs'd before; and gives it an inceſſant tendencie, or endeavour towards the lowermoſt parts of the Earth.

But as to the Example propos'd, its very eaſie to give an account of it. For ſuppoſe *A B C D*, to be a Well; wherein, by the ſtring *E F*, the Bucket is ſuſpended under water, and has its Bottom contiguous to the imaginary plain *I K*. If now we ſuppoſe the Bucket to conſiſt only of wood, lighter then water, it will not only not preſs upon the hand that holds the Rope at *E*, but will be buoyd up, till the upper parts of the Bucket be above the top of the water; becauſe the wood, whereof the Bucket is made, being lighter *in ſpecie* then water, the preſſure of the water in the Bucket *G*, and the reſt of the water incumbent on that, together with the weight of the Bucket it ſelf, muſt neceſſarily be unable to preſs the part *H* ſo ſtrongly, as the other parts of the imaginary plaine *I K* are preſs'd by the weight of the meer water incumbent on them. But if, as tis uſual, the Bucket conſiſts partly of wood, partly of iron; the Aggregate may often indeed be heavier then an equal bulk of water: But then the hand, that draws up the Bucket by the Rope *F E*, ought not, according to our Doctrine, to feel the weight of all the Bucket, much leſs that of the water contein'd in it. For though that aggregate of wood and iron, which we here call the Bucket, be heavier then ſo much water; yet it tends not downwards with its whole weight, but only with that ſurpluſage of weight, whereby by it exceeds as much water as is equal to it in Bulk ; which ſurpluſage is not wont to be very conſiderable. And as for the water in the Cavity , *G*, of the Bucket, there is no reaſon why it ſhould at all load the hand at *E*, though really the water both in the Bucket and over it do tend downwards with their full weight ; becauſe that the reſt of the water, *L I*, and *M* K, do full as ſtrongly preſs upon the reſt of the imaginary Superficies *I* K, as the Bucket and the incumbent water do upon the part *H*: and conſequently the bottom of the Bucket is every whit as ſtrongly preſs'd upwards by the weight of the water, upon all the other parts of the plain *I* K; as it tends downwards, by virtue of the weight of the Incumbent water, that is partly in the Bucket, and partly above it ; and ſo theſe preſſures ballancing one another , the hand that draws the

Rope at *E*, has no more to lift up then the ſurpluſage of weight, whereby the empty Bucket exceeds the weight of as much water as is equall in bulk (I ſay, not to the Bucket as 'tis a hollow Inſtrument, but) to the wood and iron whereof the Bucket conſiſts.

And becauſe this Example of the lightneſs of fil'd Buckets within the water has for ſo many Ages gain'd credit to, if it have not been the only ground of, the aſſertion, That water weighs not in its own Element, or in its proper place; I ſhall add (though I can ſcarſe preſent it to ſuch a *company* as this without ſmiles) an Experiment that I made to convince thoſe, that were, through unskilfulneſs or prejudice, indiſpos'd to admit the Hydroſtatical account I have been giving of the phænomenon. I took then a round wooden Box, which I ſubſtituted in the room of a Bucket; and (having fill'd it with melted

Butter, into which, when it was congeal'd, ſome ſmall bitts of lead were put, to make it a little heavier then ſo much water,) I caus'd a ſmall ſtring of twin'd ſilk to paſs through two ſmall holes, made in the oppoſite parts of the upper edge of the box, and to be ſuſpended at one end of the beam of a pair of Gold-ſmiths Scales; and then putting it into a veſſel full of water, till it was let down there, to what depth I pleas'd, it appear'd that not only the leaſt endeavour of my hand would either ſupport it, or tranſport to and fro in the water, or draw it up to the top of it; and this, whether the box were made uſe of, or whether the butter and lead alone, without the box, were ſuſpended by the ſilken ſtring: but (to evince, that it was not the ſtrength of my hand, or the ſmallneſs of the immers'd body, that kept me from feeling any conſiderable reſiſtance,) I caſt ſome grains into the ſcale that hung at the other end of the above mention'd Beame, and preſently rais'd the Lead and Butter to the ſurface of the water. So that unleſs the Schoolmen will ſay that the butter & lead were in their own Element; we muſt be allow'd to think, that the eaſie ſuſtentation, and elevation of the box, did not proceed from hence, That thoſe bodyes weigh'd not becauſe they were in their natural place. And yet in this caſe, the effect is the ſame with that which happens when a bucket is drawing out of a well.

And, to manifeſt that 'twas the preſſure of the water againſt the lower part of the ſurface of our ſuſpended body, that made it ſo eaſie to be ſupported in the water, or rais'd to the top of it; I ſhall add, that though a few grains ſuffic'd to bring the upper ſurface of the butter to the top of the water:

yet afterwards there was a conſiderable weight requiſite, to raiſe more & more of its parts above the waters ſurface ; & a conſiderabler yet, to lift the whole body quite out of the water. Which is very conſonant to our Doctrine. For, ſuppoſe the bucket to be at the part *N*, half in and half out of the water : the hand or counterpoiſe, that ſupports it in that poſture, muſt have a far greater ſtrength then needed to ſuſtein it, when it was quite under water ; becauſe that now the imaginary plain *P Q*, paſſing by the bottom of the bucket, has on its other parts but a little depth of water, as from *L* to *P*, or *M* to *Q*, and conſequently the bottom of the bucket, H, will ſcarce be preſs'd upwards above half as ſtrongly as when the bucket was quite under water. And if it be raiſed to *O*, & conſequently quite out of the water ; that liquor reaching no longer to the bottom

of

of the bucket, can no longer contribute to its ſupportation; and therefore a weight not only equal, but ſomewhat ſuperiour to the full weight of the bucket, and all that it contains, (being all ſuppos'd to be weighd in the aire,) will be neceſſary to lift it clear out of the water.

But to dwell longer on this ſubject cannot but be tedious to thoſe that have been any thing attentive to the former Diſcourſes. I proceed therefore to our Authors ſixth Argument, which is,

Object. 6. That Horſe-hairs, which are held to be of the ſame gravity with water, keep whatever place is given them in that Liquor; nor are depreſs'd by the weight of the ſuper-incumbent water.

Anſw. Whether the matter of fact be ſtrictly and univerſally true, is ſcarce worth the examining, eſpecially ſince we find the difference in point of ſpecifick gravity, betwixt moſt Horſe-haires, and moſt waters, to be inconſiderable enough. But the phænomenon, ſuppoſing the truth of it, is very eaſily explicable, according to the Doctrine above deliver'd. For ſuppoſing in the laſt Scheme the body, *R*, to be bulk for bulk exactly equiponderant to water; 'tis plain there is no reaſon why that body ſhould preſs the part *S*, of the imaginary Superficies *I* K, either more or leſs then that part *S* would be preſs'd, if, the body *R* being annihilated or remov'd, it were ſucceeded by a parcel of water of juſt the ſame bulk and weight. And conſequently, though all the water directly above the ſolid *R* do really lean upon that body, and endeavour to depreſs it; yet that endeavour being reſiſted by an equal and contrary endeavour, that proceeds (as we have been but too often faine to declare) from the preſſure exercis'd upon the other parts of the Superficies, *I* K, by the water incumbent on them; the body, *R*, will be neither depreſs'd nor rais'd. And its caſe being the ſame in what part of the water ſoever it be plac'd, provided it be perfectly environ'd with that Liquor; it muſt keep in the water (which in this whole Diſcourſe we ſuppoſe to be Homogeneous as to gravity) the place you pleaſe to give it.

And, (to add That on this occaſion) though Mathematicians have hitherto contented themſelves to prove, that in caſe a Body could be found or provided, that were exactly equiponderant to water, it would retaine any aſſignable place in it; yet the Curioſity we had, to give an Experimental proof of this Truth, at length produc'd ſome glaſs Bubbles, which ſome Gentlemen here preſent have not perhaps forgot, that were (by a dexterous hand we employ'd about it) ſo exquiſitely pois'd, as, to the wonder of the Beholders, to retain the places given them, ſometimes in the middle, ſometimes near the top, & ſometimes near the bottom of the water (though that were Homogeneous) for a great while, till ſome change of conſiſtence or gravity in the water, or ſome of its parts, made the bubble riſe or fall.

The Application of this, to what has been objected concerning Horſe-hairs, being too eaſie to need to be inſiſted on, there remains to be diſpatched our Authors ſeventh and laſt Argument; which is this.

Object. 7. *That, otherwiſe, all the inferiour parts of the water would be in perpetual motion, and perpetually expell'd by the Superior.*

Anſw. But if, by the inferior parts, he means, ſuch portions as are of any conſiderable bulk; the Anſwer newly made to the laſt objection (where we ſhew'd that the body, *R*, would retain its place any where in the water, and conſequently near the bottome) will ſhew the invalidity of this Objection. And unleſs we knew of what bigneſſe and ſhape the Corpuſcles of water are, it would perhaps be to little purpoſe to diſpute how far it may be granted, or may be true in the particles that water is made up of. Onely this I ſhall add, That, whereas this Learned Authour mentions it as an abſurdity, that the lower parts of water ſhould be in perpetual motion: And *Stevinus* himſelf, in the beginning of his Hydroſtatical Elements, ſeems to me to ſpeak ſomewhat inconſiderately of this matter; and though, as I lately ſaid, I allow ſuch ſenſible bodies, as thoſe whoſe gravity in water Writers are wont to diſpute of, to be capable of retaining their places in water, if they be *in ſpecie* equiponderant to it: Yet I am ſo far from thinking it abſurd, that the inferiour Corpuſcles of water ſhould be perpetually in motion; that I ſee not how otherwiſe they could conſtitute a Fluid body, That reſtleſs Motion of their parts, being one of the generaleſt Attributes of Liquors; and being, in water, though not immediately to be *ſeen*, yet to be eaſily *diſcover'd* by its Effects: As, when Salt, being caſt into water, the aqueous parts that are contiguous to it, and conſequently near to the bottom, do ſoon carry up many of the ſaline ones, to the very top of the water; where, after a while, they are wont to diſcloſe themſelves in little floating grains of a Cubical ſhape.

But, of this reſtleſs motion of the parts of Liquors having profeſſedly treated elſewhere already; I ſhall add nothing at preſent: But rather take notice

In the Hiſtory of fluidity & firmneſs

notice of what our Authour ſubjoyns to the laſt of his Arguments, (as the Grand thing which they ſuppoſe) in theſe words, *Ratio porro, a priori, hujus ſententiæ videtur eſſe, quia res non dicitur gravitare niſi quatenus habet infra ſe Corpus levius ſe in ſpecie.* The erroniouſneſs of which conceit, if I ſhould now go about ſolemnly to evince; I as well fear it would be tedious, as I hope it will be needleſs to thoſe, that have not forgot what may concern this ſubject in the former part of the now at length finiſh'd diſcourſe; and eſpecially where I mention thoſe Experiments, which ſhow, That neither a ſtone, nor Gold it ſelf, when plac'd deep under water, would ſinck in it, if the Superiour water, that gravitates on it, did not contribute to its depreſſion.

APPENDIX II.

Concerning the Reaſon why Divers, and others who deſcend to the Bottome of the Sea, are not oppreſs'd by the weight of the incumbent water.

AMongſt the difficulties that belong to the Hydroſtaticks, there is one which is ſo noble, and which does ſtill ſo much both exerciſe and poſe the wits of the Curious, That perchance it will not be unacceptable, if to the former Experiments we add, by way of Appendix, one that may conduce to the ſolving of this difficult problem; *viz.* Why men, deep under water, feel no inconvenience by the preſſure of ſo great a weight of water as they are plac'd under ?

The common Anſwer of Philoſophers and other Writers to this puzling Queſtion, is, That the Elements do not gravitate in their own proper places; and ſo, water in particular has no gravitation upon water, nor conſequently upon bodies every way ſurrounded with water. But that this Solution is not to be admitted, may be eaſily gather'd from our proofs of the firſt Paradox, and from divers other particulars, applicable to the ſame purpoſe, that may be met with in the foregoing papers.

A famous VVriter, and, for ought I know, the Recenteſt (except *Monſieur Paſchal*) that has treated of Hydroſtaticks, having rendred this Reaſon of the Phænomenon.

[*The Superior parts of conſiſtent water* (as he ſpeaks) *preſs not the inferior, unleſs*

leſs beneath the inferior there be a Body lighter in ſpecie then water; and therefore, ſince a humane Body is heavier in ſpecie then water, it is not preſs'd by the incumbent water, becauſe this does not endeavor to be beneath a humane Body.] He ſubjoyns, contrary to his Cuſtome, this confident Epiphonema, *Qui aliam cauſam hujus rei aſſignant, errant & alios decipiunt.*

But, by his favour, notwithſtanding this confidence, I ſhall not ſcruple to ſeek another Reaſon of the Phænomenon. For I have abundantly prov'd, that (contrary to the Aſſertion on which his Explication is built) the upper parts of water preſs againſt the lower, whether a body heavier or lighter *in ſpecie* then water be underneath the lower. And, the contrary of which being the πρῶτον ψεῦδος in this Controverſie, perhaps the matter may be ſomewhat cleared, by mentioning here a diſtinction, on, which I ſometimes make uſe of. I conſider then a body may be ſaid to gravitate upon another body in two ſenſes. For ſometimes it actually ſincks into, or gets beneath the body that was under it, as a ſincking ſtone gravitates upon water, and which I call Prævalent, or ſucceſsful Gravitation; & ſometimes it does not actually, at leaſt not viſibly deſcend, but only exerciſes its gravitation by preſſing againſt the ſubjacent body that hinders its deſcent; as when a VVoman carries a Paile of water on her head, though the weight do not actually get nearer the Center of the Earth; yet actually preſſes with its whole gravity upon the Womans head, and back, and other ſubjacent parts that hinder its actual deſcent; and according to this Doctrine I cannot admit our Authors reaſoning, that becauſe a mans body is bulk for bulk heavier then water, therefore the water does not endeavour to place its ſelf beneath it. For water, being a heavy body, derives from the cauſe of its gravity, (what ever that be) an inceſſant endeavour towards the Center of the Earth; nor is there any Reaſon, why its happening to be incumbent on a body heavier *in ſpecie* then it ſelf, ſhould deſtroy that endeavour. And therefore, though it may be ſaid that the water does not endeavour to place it ſelf beneath a humane body, becauſe indeed an inanimate Liquor cannot properly be ſaid to act for this or any other end; yet the water being a heavy body, tends continually towards the lower part of the Earth; and therefore will get beneath any body that is plac'd betwixt it and that, (without regard whether the inferior body be heavier or lighter *in ſpecie* then it ſelf) as far as the degree of its gravity will enable it; nor would it ever reſt, till it have reach'd the lowermoſt parts of the Earth, if the greater ponderouſneſs of the earth and other heavy bodies did not hinder, (not its endeavour downwards, nor its preſſure upon ſubjacent bodies, but only) its actual deſcent.

This Learned Author himſelf tells us, (as well as *Stevinus*, and others, that have written of the Hydroſtaticks, unanimouſly teach,) that if the bottom of a veſſel be parallel to the Horizon, the weight of water, that reſts upon it, is equal to a pillar of water, having that bottome for its Baſis, and for its height a perpendicular reaching thence to the uppermoſt Surface of the water. Nor is it reaſonable to conceive that there will be any difference in this preſſure of the incumbent water, whether the bottom be of Deale that will ſwimme, or of Box that will ſinck in water; or to ſpeak more generally, whether it be of Wood, *in ſpecie* lighter then water, or of Copper, or ſome other Metal, that is *in ſpecie* heavier then it. And ſince water,being not a ſolid Body, but a fluid, conſiſts (as other fluids) of innumerable Corpuſcles , that , though extreamly minute, have their own ſizes and figures ; And ſince the preſſure of water upon the bottom of a veſſel is proportionate to its perpendicular height over the bottom ; 'Tis manifeſt, that the upper Corpuſcles preſs the bottom as well as the lower ; which, ſince they cannot do immediately, they muſt do by preſſing the intermediate ones. And I have already ſhown (diſcourſing one of the former Paradoxes,) that the Superior parts of water do not onely preſſe thoſe that are directly under them, but communicate a preſſure to thoſe that are aſide of them, and at a diſtance from them.

And if it be objected, That water

endeavours to get beneath a Bottome of Glaſs Veſſels, or other bodies heavier *in ſpecie* then its ſelf, becauſe under that bottome there is aire, which is a body lighter *in ſpecie* then water: I ſay, that this is precarious; for the indiſputable gravity of the water is alone ſufficient to make it always tend downwards, (though it cannot always move downwards) what ever body be beneath it. And who can aſſure the makers of this Objection, That there are not beneath even the bottome of Rivers, or of the Sea, (where yet they ſay water is conſiſtent, and reſts as in its own place,] vaſt ſpaces repleniſhed but with aire, fumes, or fire, or ſome other body lighter then water? For, (not to mention that the Carteſians take the Earth we tread on, to be but a thin Cruſt of the Terreſtrial Globe, whoſe inſide, as farre as the Center, is repleniſh'd with a ſubtle fluid matter, like that whereof the Sunne conſiſts.) We know that in ſome places, as particularly at a Famous Coal-mine in *Scotland*, there are great Cavities that reach a good way under that ground that ſerves there for a bottome to the Sea: So that, for ought theſe Objectors know, ev'n according to their own Doctrine, the water ev'n in the Sea, may endeavour to get beneath a body heavier *in ſpecie* then it ſelf.

But, for my part, I cannot but think, that, to imagine the water knows, whether or no there be aire or ſome lighter body then its ſelf beneath the body it leans on, and the ſuperior parts do accordingly exerciſe or ſuſpend their preſſure upon the inferior; is to forget that it is a heavy Liquor, and an inanimate Body.

Another Solution there is of this Hydroſtatical problem, we have been diſcourſing of, which I met with in a Printed Letter of Monſieur *Des Cartes*, in theſe terms.

Je ne me, &c. *I remember not what reaſon 'tis that* Stevinus *gives, why one feels not the weight of water, when one is under it: but the true one is, that there can no more of water gravitate upon the body that is in it, or under it, then as much water as could deſcend in caſe that body left its place. Thus for Example: If there were a Man in the Barrel*, B, *that ſhould with his Body ſo ſtop the hole*, A, *as to hinder the waters getting out, he would feel upon himſelf the weight of the whole Cylinder of water*, A B C, *of which I ſuppoſe the Baſis to be equal to the hole* A; *for as much as if he ſunck down through the hole, all the Cylinder of water would deſcend too, but if he be a little higher, as about* B, *ſo that he does no longer hinder the water from running*

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Fig. 22.

out at the hole A, *he ought not to ſeel any weight of the water which is over him, betwixt* B *and* C, *becauſe if he ſhould deſcend toward* A, *that water would not deſcend with him, but contrarywiſe a part of the water which is beneath him towards* A, *of equal bulk to his Body, would aſcend into its place: ſo that in ſtead of feeling the water to preſs him from the Top downward, he ought to feel that it buoys him upward from the bottome; which by Experience we ſee.*

Thus far this ſubtil Philoſopher: for whoſe Ratiocinations though I am wont to have much reſpect, yet I muſt take the liberty to confeſs my ſelf unſatisfy'd with this. For haveing already ſufficiently prov'd, That the upper parts of water preſs the lower, and the bodies plac'd beneath them, whether ſuch bodies be lighter *in ſpecie* then water or heavier; we have ſubverted the Foundation, upon which Monſieur *Des Cartes*'s ingenious, though unſatisfactory, Explication is built. And yet I ſhall add *ex abundanti*, That ſuppoſing what he ſayes, That in caſe the ſolid *B* ſhould deſcend towards *A*, the incumbent water would not deſcend with it, but a part of the ſubjacent water, equal in bulk to the ſolid, would aſcend, and ſucceed in its room ; yet that is but accidental, by reaſon of the ſteinchneſs and fulneſs of the Veſſel. And though indeed the Superior water cannot actually deſcend upon the depreſſion of the ſolid at *B*, if, at the ſame time while that body deſcends, an equal bulk of water ſucceeds in its place : Yet both the ſolid about *C*, and the water that ſucceeds it, do, in their turns, hinder the deſcent of the Superior water ; which therefore muſt gravitate upon which ſoever of the two it be that actually comes to be plac'd directly under it, if there be nothing, before the diſplacing of the ſolid, capable to take away the natural gravity, upon whoſe account the water, over *B* and *C*, does inceſſantly tend downwards. And though Monſieur *Des Cartes* does not ſo clearly expreſs himſelfe, whether he ſuppoſes the hole at *A* to be ſtopp'd with ſome other body, when the ſolid is plac'd about *B* : yet, becauſe he is wont to ſpeak conſiſtently, I preſume he means, that when the ſolid is remov'd to *B*, the hole at *A* is otherwiſe ſufficiently ſtopp'd ; I ſay then, that the reaſon why the ſolid, which, whilſt at *A*, ſuſtain'd a great preſſure from the incumbent water, feels not the weight of it, when plac'd at *B*, is not that which *Monſieur des Cartes* gives, but this, That the ſolid being environ'd with water, the ſubjacent water does (as we have often had occaſion to manifeſt) preſs it upwards, full as ſtrongly (and ſomewhat more) as the weight of the incumbent water preſſes it downwards; So that a mans body, in ſtead of ſincking, would be buoy'd up; if, as it is a little heavier, it were a little lighter *in ſpecie* then water. Whereas, when the ſolid was that alone which cover'd and ſtop'd the hole, there was a manifeſt Reaſon why it ſhould be forcibly thruſt downwards by the weight of the incumbent water B *C*. For, in that caſe, there was no water underneath it at *A*, to ſupport the ſolid; and, by its preſſure upward, to enable it to reſiſt ſo great a weight.

And this, (to hint that upon the by) may perchance help us to gueſs at the reaſon of what Geographers relate of the Lake *Aſphaltites* in *Judæa*, (in caſe the matter of fact be true,) That this dead Sea (as they alſo call it) will not ſuffer any living creature to ſinck in it. For the Body of a Man (and for ought we know of other Animals,) is not much heavier *in ſpecie* then common freſh water: Now if in this Lake (that ſtands where *Sodom* and *Gomorrah* did, before thoſe impious Regions were deſtroy'd by fire from Heaven,) we ſuppoſe, (which the nature of the Soyle, and the Sacred Story makes probable enough) That the water abounds with Saline or Sulphurous Corpuſcles; (the former helping the later to aſſociate with the water, as we ſee in ſope conſiſting of ſalt and oyle, and in Chymical mixtures of Alcalis and Brimſtone diſſoluble in water) the Liquor may have its gravity ſo augmented, as to become heavier *in ſpecie* then the body of an animal. For I have learned of a Light Swimmer, that he could hardly begin to Dive in ſalt water, though he eaſily could in freſh. And 'tis not difficult to make a Brine or *Lixivium* (which are but Solutions of ſalt in water,) heavy enough to keep up an egg from ſincking. And, not only barely by diſſolving a metalline body in a ſaline *Menſtruum*, without otherwiſe thickning the Liquor, I have brought ſolid pieces of Amber it ſelf to ſwim upon it: but I have try'd that certain ſaline Solutions, which I elſewhere mention; nay, and a diſtill'd Liquor, (I us'd defleam'd oyle of Vitriol) without any thing diſſolv'd in it, would do the ſame thing; by reaſon of the numerous, though minute, Corpuſcles of ſalt and ſulphur, that it abounds with.

There remains but one ſolution more of our Hydroſtatical probleme, that I think worth mentioning, and that is given by the Learned *Stevinus* in theſe words,

Omni preſſu quo Corpus dolore afficitur, pars aliqua Corporis luxatur; ſed iſto preſſu

preſſu nulla Corporis pars luxatur, iſto igitur preſſu Corpus dolore nullo afficitur. Aſſumptio ſyllogiſmi manifeſta eſt, nam ſi pars aliqua, ut caro, ſanguis, humor, aut quodlibet deniq; membrum luxaretur, in alium locum concedat neceſſe eſſet: atqui locus ille non eſt extra Corpus; cum aqua undiquaque æquali preſſu circumfuſa ſit (quod vero pars ima, per 11. *propoſitionem* Hydro*ſtaticorum, paulo validius prematur ſuperiori, id hoc caſu nullius momenti eſt, quia tantula differentia partem nullam ſua ſede dimovere poteſt) neque item intra ipſumCorpus concedit, cum iſtic Corpore omnia oppleta*

Stevinus Hydroſtat. Lib. 5. *pag.* 149.

Sed Exemplo clarius ita intelliges, eſto ABCD *aqua, cujus fundum* D C, *in quo foramen* E *habeat Epiſtomium ſibi inſertum, cui Dorſo incumbat Homo* F, *Quæ cum ita ſint, ab aquæ pondere ipſi inſidente nulla pars Corporis luxari poterit, cum aqua, ut dictum eſt, undiquaque æqualiter urgeat.* Fig. 23.

Si vero ejus veritatem explorare libeat, eximito Epiſtomium, tumque tergum nulla re fultum ſuſtinebitur, ut in locis cæteris, ideoque iſtic tanto preſſu afficietur, quantus tertio exemplo ſecundæ propoſitionis hujus demonſtratus eſt: vid. *quantum efficit columna aquea cujus Baſis ſit foramen* E. *altitudo autem eadem quæ aquæ ipſi inſidentis. Quo exemplo propoſiti veritas manifeſte declaratur.*

ſint, unde ſingulæ partes ſingulis partibus æqualiter reſiſtunt, namque aqua undiquaque eadem ratione Corpus totum circumſtat. Quare cum locus is nec intra, nec extra Corpus ſit; abſurdum, imo impoſſibile fuerit, partem ullam ſuo loco emoveri, ideoque nec Corpus hic afficitur dolore.

This Solution of *Stevinus*, I eſteem preferrible by farr, to thoſe that are wont to be given of this difficult Probleme: But yet, the Phænomenon ſeems to me to have ſtill ſomewhat in it of ſtrange. 'Tis true, that if the Queſtion were only that which ſome put, *viz.* Why the body of a Diver, when it is near the bottom of the Sea, is not preſs'd down by ſo vaſt a weight of water, as is incumbent on it; It might be rationally anſwer'd, That the weight of ſo much water, as leans upon the body, is not ſuſtein'd by the force of the body it ſelf, but by that of the water which is under it. For, by the Experiments and Explications, we have annexed to ſome of the foregoing Paradoxes, it appears, That the ſubjacent water, by its preſſure upwards, is able, not only to ſupport the weight of the incumbent water, but ſo far to exceed it, that it would not only ſupport the immers'd body, and the incumbent water, but buoy up the body, if it were never ſo little lighter *in ſpecie* then water. And as for what *Stevinus* inſinuates, That, when the water preſſes the body every way, that preſſure is not felt, though it would be, in caſe it preſs'd upon ſome parts, and not upon others; I am of the ſame opinion too; and, to prove it, ſhall not make uſe of the example he propoſes, in the words immediately following thoſe of his, I juſt now recited: (For I doubt, that example is rather a ſuppoſition, then a try'd thing;) but by an Experiment which may be eaſily made, and has diverſe times been ſo, in our Pneumatical Engine. For, though the aire be a heavy fluid, and though, whilſt it uniformely preſſes the whole ſuperficies of the body, we feel not the preſſure of it. And though, for this reaſon, you may lay the palm of your hand upon the open orifice of a ſmall braſs Cylinder, apply'd to the Engine in ſtead of a Receiver, without any hurt; Yet when, by pumping, the aire that was before under the palm of your hand, is withdrawn, and conſequently can no longer help to ſupport your hand, againſt the preſſure of the external and incumbent aire; the external aire will lean ſo heavy upon the back of your hand, that you will imagine ſome ponderous weight is lay'd upon it. And I remember by ſuch an Experiment, I have not onely had my hand put to much pain, but have had the back of it ſo bent downward, as if it were going to be broken.

But though ſuch conſiderations, as theſe, may much leſſen the difficulty of our phænomenon, whoſe cauſe is inquired into; Yet ſtill it ſeems ſomewhat odd to me, That (ſince 'tis evident from the nature of the thing, and by *Stevinus*'s his confeſſion, that there is a vaſt preſſure of water againſt every part of the body, whoſe endeavour tends inward,) ſo exceedingly forcible a preſſure, (which thruſts, for inſtance, the Muſcles of the Arms and Thighs againſt the Bones, the Skin and Fleſh of the Thorax againſt the Ribs,) ſhould not put the Diver to any ſenſible pain; As I find not (by one that I examin'd) that it dos; (Though this man told me, he ſtay'd a good while at the depth of betwixt 80 and 100 foot under the Sea water, which is heavier then freſh water;) For, that which *Stevinus*'s Explication will only ſhow is, That there muſt be no manifeſt diſlocation of the greater parts of the Body; whereas the bare compreſſion of two ſmall parts, one againſt another, is ſufficient to produce a ſenſe of pain.

But it ſeems, the Texture of the bodyes of Animals is better able to reſiſt the preſſure of an every way ambient fluid, then, if we were not taught by experience, we ſhould imagine. And therefore, to ſatisfie thoſe that (ſecluding the Queſtion about the ſenſe of pain,) think it an abundantly ſufficient Argument, (to prove, that bodyes immers'd under water, are not compreſs'd by it,) That *Divers* are not oppreſs'd, and ev'n cruſh'd by ſo vaſt a load of water, (amounting, by *Stevinus*'s computation, to many thouſands of pounds) as is incumbent on them. We will add, that though an Experiment, propos'd by Monſieur *Paſchal* to this purpoſe, were ſuch, that at firſt ſight I ſaid that it would not ſucceed, (and was not upon tryal miſtaken in my conjecture;) yet it gave me the occaſion to make another, which will, I hope, fully make out the thing I deſign'd it for.

The Ingenious Monſieur *Paſchal* would perſwade his Readers, that if into a glaſs Veſſel, with luke-warm water in it, you caſt a flie; and, by a Rammer, forcibly preſs that water, you ſhall not be able to kill, or hurt the flie. VVhich, ſays he, will live as well, and walk up and down as lively, in luke-warme water, as in the aire. But, upon tryal with a ſtrong flie, the Animal was (as we expected,) preſently drowned, and ſo made moveleſs, by the luke-warm water.

Wherefore we ſubſtituted another Experiment, that we knew would not only ſucceed, (as you will preſently ſee it will do,) but teach us how great a preſſure the included Animal muſt have been expos'd to. VVe took then a ſomewhat ſlender Cylindrical pipe of Glaſs, ſeal'd at one end, and open at the other; and to this we fitted a Rammer, which (by the help of ſome thongs of ſoft leather, that were carefully wound about it) did ſo exactly fill the pipe that it could not eaſily be mov'd to and fro; and would ſuffer neither water, nor aire, to get by betwixt it, and the internal ſurface of the Glaſs. VVe alſo provided ſome ſmall Tad-poles (or *Gyrini*) about an Inch long or leſs; which ſort of Animals we made choice of before any other, partly becauſe they could, by reaſon of their ſmalneſs, ſwim freely to & fro in ſo little water as our pipe contain'd; & partly becauſe thoſe Creatures, being as yet but in their Infancy, were more tender, and, conſequently, far

Fig. 24.

more expoſ'd to be injur'd by compreſſion, then other Animals of the ſame Bulk, but come to their full age and growth, would be, (as indeed ſuch young Tad-poles are ſo ſoft and tender, that they ſeem, in compariſon to the bigger ſort of flies, to be but organiz'd Gelly.) One of theſe Tadpoles being put into the water, and ſome Inches of aire being left in the pipe, for the uſe anon to be mention'd ; the water and aire, and conſequently the Tadpole, were by the intruſion of the plug or rammer, with as great a force as a man was able to imploy, violently compreſs'd ; and yet, though the Tadpole ſeem'd to be compreſs'd into a little leſs Bulk then it was of before, it ſwom freely up and down the water, without forbearing ſometimes to aſcend to the very top , though the Inſtrument were held perpendicular to the Horizon. Nor did it clearly appear to us, That the little Animal was injur'd by this compreſſion ; and moſt manifeſt it is, he was not cruſh'd to death, or ſenſibly hurt by it.

And having repeated this Experiment ſeveral times,& with Tadpoles of differing ages; we may, I preſume, ſafely conclude, That the Texture of Animals is ſo ſtrong, that, though water be allowed to weigh upon water, yet a *Diver* ought not to be oppreſt by It: Since, whether or no water weighs in water, 'tis manifeſt that in our Experiment, the water, and conſequently the Tadpole, was very forcibly by an External Agent compreſs'd betwixt the violently condens'd aire, and the rammer. And, by the notice we took of the quantity of aire before the compreſſion began, and that to which it was reduc'd by compreſſion ; The moderateſt eſtimate we could make, was, That it was reduc'd into an *eighth*, or *tenth* part of it's former ſpace; and ſo (according to what we have elſwhere prov'd) the preſſure that was upon the aire, (and conſequently upon the water, and the included Tadpole,) was as great as that of a Cylinder of water of above 200 if not 300 foot high. And yet all this weight being unable to oppreſs, or ſo much as manifeſtly to hurt, the tender Tadpole (which a very ſmall weight would ſuffice to have cruſh'd, if it preſt only upon one part of it, and not upon the other) we may thence learn the Truth of what we have been endeavouring to evince: That though water be allowed to preſs againſt water, and all immers'd Bodys; yet a *Diver* may very well remaine unoppreſs'd at a great depth under water, as long as the preſſure of it is uniforme againſt all the parts expos'd thereunto.

FINIS.

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